# Do rating agencies deserve some credit? Evidence from transitory shocks to credit risk☆

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## Abstract

We find that Credit Rating Agencies (CRAs) can discern transitory shocks to credit risk that stem from fire sales by mutual funds, while Credit Default Swap (CDS) markets cannot. CRAs are significantly less likely to downgrade firms that experience fire sales than characteristics-matched controls, while CDS spreads increase by similar amounts for both. These results explain why ratings are useful despite the availability of market-based estimates of credit risk: the ability to ignore transitory shocks is valuable because rating changes have real consequences for private contracts and access to capital.

*Keywords:* Credit Ratings, Mutual Funds, Institutional Investors, Financial Intermediation

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**Abstract**

We find that Credit Rating Agencies (CRAs) can discern transitory shocks to credit risk that stem from fire sales by mutual funds, while Credit Default Swap (CDS) markets cannot. CRAs are significantly less likely to downgrade firms that experience fire sales than characteristics-matched controls, while CDS spreads increase by similar amounts for both. These results explain why ratings are useful despite the availability of market-based estimates of credit risk: the ability to ignore transitory shocks is valuable because rating changes have real consequences for private contracts and access to capital.

Credit ratings agencies (CRAs) have historically played an important role as information intermediaries in financial markets. However, CRAs are now under siege. A vast academic literature finds that the issuer-pays model and competitive pressures distort the incentives of CRAs to issue accurate ratings.[1] Regulators and other observers have pointed to inflated ratings as a key cause of the mortgage securitization boom of the early 2000s and the subsequent recession.[2] The Dodd-Frank Act now requires regulatory agencies to remove all references to CRAs from regulations, thereby limiting regulatory uses of ratings. Finally, research finds that estimates of default probability that include information from equity (Hilscher and Wilson, 2016) or Credit Default Swap markets (Chava, Ganduri and Ornthanalai, 2016) are more accurate or timely than ratings. In fact, Flannery, Houston and Partnoy (2010) argue that both regulators and private investors should use market-based estimates of credit risk instead of credit ratings.

Nevertheless, CRAs continue to thrive. In 2014, revenues at the two largest CRAs, Moody's and Standard & Poor's, surpassed pre-crisis levels with gross margins of 40%–50%. Thus, despite their flaws and diminished regulatory relevance, CRAs appear to pass the market test. But, how do CRAs add value in a world where accurate market-based estimates of credit risk are easily available?

CRAs claim that they add value because market-based estimates of credit risk are noisy and this noise can have real effects. For example, Cantor and Mann (Moody's; 2006) state:

> Our conversations with investors, issuers and regulators have led us to conclude that many market participants have a strong preference for credit ratings that are not only accurate but also stable. They want ratings to reflect enduring changes in credit risk because rating changes have real consequences—due primarily to ratings based portfolio governance rules and rating triggers—that are costly to reverse. Market participants, moreover, do not want ratings that simply track market-based measures of credit risk. Rather, ratings should reflect independent analytical judgments that provide counterpoint to often volatile market-based assessments.

In this paper, we investigate whether CRAs actually do what they say. Can CRAs distinguish between permanent ("enduring") and transitory shocks to credit risk in real time? Note that the stability of ratings,

[1] See for example, Griffin, Nickerson and Tang (2013) and Becker and Milbourn (2011), and other references in footnote 8.

[2] See for example, the Financial Crisis Inquiry Commission Report, and SEC Commissioner Luis A. Aguilar's public statement "Restoring Integrity to the Credit Rating Process" on August 27, 2014.

by itself, does not imply that CRAs add value. Market-based estimates of credit risk can be smoothed to be as stable as desired by, for example, using moving averages or threshold-based rules. To add value, CRAs must achieve stability not by smoothing or merely waiting to see which shocks reverse, but by discerning which shocks are transitory in real-time. Thus, ratings must contain different information from market-based estimates if they are to be a useful "counterpoint" to market prices.

An ideal setup to test whether CRAs can discern which shocks are transitory is to consider two ex ante identical firms. An event causes market participants to perceive a similar increase in credit risk for both firms. However, the 'treated' firm's increase in risk is due to a transitory shock and the 'control' firm's is due to a permanent shock. If CRAs are able to distinguish between these two types of shocks in real-time, we expect that the treated firm will be less likely to be downgraded than the control during or soon after the event.

Our empirical tests operationalize this ideal setup. We employ shocks to equity prices as our measure of shocks to credit risk. Adverse changes in equity value can translate into changes in credit risk in two ways. First, they increase market leverage, thereby directly increasing credit risk (Merton, 1974). Second, declines in stock prices signal bad news about the firm's fundamentals (Fama, 1981; Kothari and Sloan, 1992). CRAs also state that they consider stock prices as signals in reviewing ratings (Adelson, 2008).

We use mutual fund fire sales as in Edmans, Goldstein and Jiang (2012) to identify firms with transitory shocks to equity value. Edmans, Goldstein and Jiang (2012) show that mutual fund fire sales result in economically meaningful shocks to equity prices that reverse over several quarters. We designate firms that experience fire sales in a given quarter as treated firms. Control firms have similar returns in the event quarter and are also matched by credit rating, industry, and propensity to experience fire sales at the start of the event quarter. We confirm that treated-firm returns reverse ex post, while control-firm returns do not.

Our key finding is that CRAs can in fact distinguish between transitory and permanent shocks to credit risk. Treated firms are 0.9% less likely to be downgraded than controls. This reduction is approximately half the unconditional downgrade probability of treated or control firms of 2%. The treatment effect increases to 1.5% if we also include one quarter after the fire-sale in the event period. Results are similar if we take the severity of downgrades into account. The difference in the average number of downgrade notches between treated and control firms during the fire sale and subsequent quarter is 0.049, which is approximately half the average unconditional number of downgrade notches of treated firms at 0.085.

We find even stronger results when we focus on the sample of firms in which fire sales are likely to be most salient. In a subsample with negative event quarter returns (-12% on average), the average difference in the number of downgrade notches between treated and control firms nearly doubles from 0.049 in the full sample to 0.084. Even among stocks with negative returns, we expect that results should be strongest in firms where we have the greatest confidence that the shocks are temporary. In particular, stocks whose returns exhibit the most pronounced V-shaped pattern, with the deepest drop in the event quarter and the strongest subsequent recovery, should have the strongest treatment effects. We find this is indeed the case: the difference in downgrade notches in the event quarter triples from the shallowest to steepest V-shape.

A shock to credit risk can be transitory because it is a false signal or because it is a fundamental shock that reverses quickly, and distinguishing between these two possibilities in practice is very difficult. For example, a rumor of a takeover that does not eventually occur has elements of both explanations. However, distinguishing between these possibilities is not crucial to our research question, because rating changes that reverse impose potentially avoidable costs regardless of their cause.

One concern with using fire sales to identify exogenous transitory shocks is that investors may be more likely to withdraw capital from mutual funds that they believe will perform poorly in the future. If investors behave in such a manner, stocks subject to fire sales are of worse quality, and hence likely to have greater downgrade probabilities than a typical firm, which would bias us against finding our results. Nevertheless, our empirical strategy contains two elements to mitigate such selection biases. First, as in Edmans, Goldstein and Jiang (2012), we identify fire sales using hypothetical distressed fund trades, which are computed assuming that funds sell their holdings in proportion to their portfolio weights before the extreme outflows. Although this strategy addresses selection biases arising from fund manager discretion during the event quarter, fire sale firms may be different from typical firms prior to the event quarter in terms of variables related to mutual fund ownership and past performance (Berger, 2015). To minimize observable differences between treated and control firms, we also match on the firm's propensity to experience fire sales. We find no evidence that fire-sale firms have relatively worse past performance in our sample of rated firms: past returns and downgrades do not predict fire-sales.

Our treatment-control setup precludes alternative explanations for these results that are common to all rated firms. For example, explanations that rely on coarseness (Goel and Thakor, 2015) or lack of timeliness of ratings (Chava, Ganduri and Ornthanalai, 2016) apply to both treated and control firms. Similarly, macroeconomic factors such as business cycles, which cause variation in fundamental shocks and downgrade propensities over time, also affect both sets of firms. Of course the validity of our design is contingent on the quality of the matches. We confirm that our matching procedure works well. Treated and control firms are balanced for a wide range of variables at the start of the event quarter including mutual fund ownership, size, leverage, past returns, and default probability estimated using the Campbell, Hilscher and Szilagyi (2008) model. Downgrade probabilities and returns for treated and control firms also exhibit parallel trends before the fire sale quarter.

A possible alternative explanation for these results is that credit markets actually distinguish between permanent and temporary equity price shocks, and CRAs passively follow credit markets. If true, this ability of credit markets to see through temporary shocks in equity markets may be interesting in itself; however, such behavior does not imply a special role for CRAs. Consequently, we examine whether credit markets respond to equity fire sales. In particular, we examine Credit Default Swap (CDS) markets because Blanco, Brennan and Marsh (2005) find that CDS markets lead bond markets. We find that both treated and control firm CDS spreads increase by the same amount as do CDS-implied rating downgrades. Most of the increase in spreads for both treated and control firms occurs in the quarter after the fire sale, consistent with Hilscher, Pollet and Wilson (2015), who find that information flows from equity to CDS markets. Treated firm spreads eventually revert while control firms do not, re-confirming that the fire sales shocks are indeed temporary. The sample with available CDS data is smaller than the ratings sample in the cross-section as well as the time-series and hence, it is possible that these tests do not have power. However, we find that even in the sample where CDS data exists, treated firms have significantly lower rating downgrade probability than controls.

Why do CRAs appear to see through transitory shocks to equity prices, while CDS spreads do not? A possible explanation is that CRAs ignore market prices all together and hence do not react to any price shocks. However, this seems unlikely. CRAs themselves say that they consider stock prices in evaluating credit risk. For example, Adelson (Standard & Poor's; 2008) states: "...sudden changes in the price of a company's stock sometimes signal abrupt changes in the company's fundamental condition or prospects.

Accordingly, we respond to a sudden change in stock price by exploring the underlying causes."[3]

Thus, after seeing a price shock, CRAs may seek information to determine whether there is actually a substantial decline in the firm's fundamentals that warrants a downgrade. Besides public sources, CRAs also have access to nonpublic information from firms that may not be available to CDS market participants, including "...budgets and forecasts, financial statements on a stand-alone basis, internal capital allocation schedules, contingent risks analyses and information relating to new financings, acquisitions, dispositions and restructurings."[4] The information that CRA's collect from direct sources can help them better interpret information in market prices, consistent with the predictions of the model in Bond, Goldstein and Prescott (2009). The model shows that market prices need not be completely informative about current fundamentals because they also impound information on expected actions by regulators and other economic agents and implies that a well-informed agent can infer fundamentals from market prices more accurately than an ill-informed one.

Finally, we examine whether the ability of CRAs to distinguish transitory shocks from permanent ones has changed over time. This test is informative for two reasons. First, the test shows that the effects we document are robust and not specific to a particular point in time. Second, the enactment of Reg FD with an exemption for CRAs in 2000 resulted in a change in the information advantage of CRAs (Jorion, Liu and Shi, 2005), allowing us to test whether the ability of CRAs to see through transitory shocks is related to their informational advantage.[5] We find that the difference in downgrade probabilities for treated firms relative to controls increases after the enactment of Reg FD. However, the difference is not statistically significant perhaps because of the smaller sample size of this test.

Our results survive a battery of robustness tests including changes to the matching procedure such as using a finer or coarser caliper for the propensity score match, using several neighbors instead of one, and using a finer or coarser industry classification. Results are also similar if we exclude specific industries (e.g., financials and utilities) and the period of the 2007-2009 financial crisis. We also find that the effect

[3]Other mechanisms that Adelson (Standard & Poor's; 2008) mentions are that equity price drops can affect the ability of firms to raise new capital and also impact the ability of 'confidence-sensitive' firms such as banks and brokers to operate.

[4] From Standard & Poor's November 2002 submission to the Securities and Exchange Commission, which describes the information sources available to CRAs in detail.

[5]The removal of the specific exemption of CRAs from Reg FD under the Dodd-Frank Act may not be material, because CRAs argue that they meet other criteria for exemption. In particular, they do not seek to make investment decisions based on the private information and their engagement letters with firms contain confidentiality agreements (Carbone, 2010).

is pervasive and not statistically different across rating categories and verify that our results are not driven by differences in volatility of the ratings.[6] Finally, we examine a set of placebo tests, where the treatment variable is based not on fire sales by distressed funds, but on sales by all funds, excluding fire sales. Such sales are likely to be information driven, and ex post we find that they result in permanent shocks to prices. We find no differences in rating downgrades for this 'placebo treatment', suggesting that our results are not driven by the characteristics of stocks held by mutual funds that experience outflows.

Our paper contributes to the literature on the role and impact of CRAs. Overall, the literature on CRAs finds that their actions affect market participants, but also highlights concerns about their incentives. For example, Kisgen (2007) argues that downgrades can result in significant real costs to firms including a loss of eligible investors and customers and higher costs of borrowing, and Ellul, Jotikasthira and Lundblad (2011) show that downgrades result in fire sales in corporate bonds.[7] Research on CRAs also finds that the issuer-pays compensation structure as well as regulatory and contractual reliance on ratings results in distortions in incentives for CRAs to issue accurate ratings.[8] Our results do not imply either that CRAs are free from conflicts of interest, or that ratings are more accurate than market-based estimates. Instead, we argue that because accuracy is only one part of the CRA's objective function, lower accuracy need not imply that CRAs are redundant. The other objective of CRAs—ratings stability to mitigate the adverse real effects of downgrades—is also important.

Thus, our paper contributes to research that examines the trade-off between ratings stability and accuracy (Altman and Rijken, 2004, 2006; Cornaggia and Cornaggia, 2013; Löffler, 2013). Our paper complements this research by showing that CRAs are able to distinguish between transitory and permanent shocks in real time, thereby adding value relative to smoothed market-based estimates. We thus provide an answer to why CRAs continue to thrive despite the flaws documented by prior research and the availability of substitutes. Our paper also complements Cornaggia, Cornaggia and Israelsen (2017), who find that municipal bond ratings matter for prices even without a change in fundamentals. Our results provide an explanation for why

[6] Almeida, Cunha, Ferreira and Restrepo (2017) show that rating downgrades have significant adverse effects even within high-credit-quality firms.

[7] Also see Kisgen (2009), Tang (2009), Sufi (2007), and Manso (2013).

[8] One source of distortions is the compensation structure of CRAs (Skreta and Veldkamp, 2009; Sangiorgi, Sokobin and Spatt, 2009; Bolton, Freixas and Shapiro, 2012; Griffin, Nickerson and Tang, 2013; Cornaggia and Cornaggia, 2013; Fulghieri, Strobl and Xia, 2013; Xia, 2014; Sangiorgi and Spatt, 2016). The other source of distortion is the regulatory and contractual reliance on ratings (Kisgen and Strahan, 2010; Opp, Opp and Harris, 2013; Bruno, Cornaggia and Cornaggia, 2015).

investors consider ratings informative.

Additionally, our paper is also related to the literature on the real effects of financial markets (see Bond, Edmans and Goldstein (2012) for a survey). This strain of research shows that managers and other decision-makers learn from stock prices and use this information to guide their decisions. Similar to our setup, a growing body of research employs mutual fund fire sales as transitory equity price shocks and shows that economic agents take decisions based on these non-fundamental shocks.[9] Our results suggest CRA rating policies may have evolved to mitigate the adverse real effects of financial prices.[10] If market-based estimates of credit risk are used in contracts and regulations instead of ratings, transitory shocks to asset prices could have real effects. For example if suppliers deny credit based on a transitory increase in credit risk implied from market prices, it could impact a firm's profitability and production.

Our results also imply that recent regulatory efforts in the Dodd-Frank Act to limit the privileged position of CRAs, albeit with the laudable goal of encouraging investors to do independent analysis, may have unintended consequences. Transitory shocks in financial markets are more likely to propagate to the real economy if regulations restrict the access of CRAs to private information (thereby inhibiting their ability to discern which shocks are temporary) or create disincentives for CRAs to issue independent opinions.

## 1. Data and Methodology

This section describes the datasets we use, the methodology, and construction of variables.

### *1.1. Ratings and other data*

Our dataset is based on the intersection of four databases: (i) mutual fund holdings from Thompson 13F filings, (ii) mutual fund returns and total net assets from the Center for Research in Security Prices (CRSP) Survivorship-Bias Free mutual fund database, (iii) credit ratings and firm accounting data from Compustat, and (iv) equity returns and prices from CRSP. The filters we impose on the mutual fund data follow prior research and are described in Appendix A.

[9] See Acharya, Almeida, Ippolito and Perez (2014), Ali, Wei and Zhou (2011), Derrien, Kecskés and Thesmar (2013), Phillips and Zhdanov (2013), Khan, Kogan and Serafeim (2012).

[10] Other institutions may also play a similar role. For example, Sulaeman and Wei (2012) find that a subset of skilled equity analysts are able to issue price-correcting recommendations for stocks subject to flow-driven mispricing.

We use data on Standard and Poor's (S&P) issuer ratings in our main tests, but also provide robustness results for Moody's ratings in the Appendix.[11] We translate each letter rating into a numerical rating, so that a one unit increase reflects a one notch improvement of rating (e.g. from BBB+ to A). We also obtain Credit Default Swaps (CDS) data from Markit. As described in Appendix B, we use the 5-year contract with the document clause that is likely to be the most liquid CDS contract on that stock. Our measure of CDS spreads each month is the mean CDS spread over the last five trading days that month.[12] We also use CDS implied downgrades from Markit, which are based on ratings computed only using CDS spreads by Markit. Finally, for each stock-quarter we compute the 12-month ahead default probability following Campbell, Hilscher and Szilagyi (2008) (henceforth, CHS). Other variables are standard and defined in Appendix B.

### *1.2. Methodology*

Our goal is to test whether credit rating agencies can distinguish between transitory and permanent shocks to credit risk. To do so, we use a matched sample, difference-in-difference methodology. Treated firms are those that experience fire sales in a given quarter. Matches have similar characteristics as treated firms at the start of, and similar returns during, the fire-sale quarter. Note that the returns of matches are not due to fire sales and are presumably permanent (we confirm that they do not reverse in the data). We test whether realized downgrade probabilities are different for treated firms relative to controls over the fire sale and subsequent quarters. This is a 'difference-in-difference' test in that it is the difference in the change in credit ratings between treated and matched firms over the event and subsequent quarter.

The fire-sale approach is motivated by the observation that while mild fund outflows can be absorbed by a fund's cash position, extreme outflows are more likely to force managers to liquidate stocks, thereby generating price pressure on these stocks. Coval and Stafford (2007) show that stocks subject to fire sales suffer a substantial decline in prices that is transitory. Edmans, Goldstein and Jiang (2012) refine the approach in Coval and Stafford (2007) to address a potential source of endogeneity: mutual fund managers choose which stocks to sell and their selection criteria may be linked to the outcome variable. Hence (as discussed in further detail below), they use trades implied by a fund's portfolio weights and outflows rather

[11] We focus on S&P because our sample of Moody's data is shorter and has a lower match rate with CRSP.

[12] Results are similar if we use the last day or the mean spread over the entire month. We report results based on the mean over the last five days, because the last day's price is more volatile, and the mean over the entire month is stale relative to stock returns based on end-of-month prices.

than actual trades. We follow the approach in Edmans, Goldstein and Jiang (2012) to identify fire sales. We confirm that in our sample, the shocks to treated firms are temporary, while the shocks to controls are not. Treated firms experience negative returns that revert back over the next few quarters, while controls exhibit similar returns that do not reverse. We therefore follow the literature in referring to the fire-sale shocks as 'transitory'.

The next step is to identify a set of firms that serve as controls. Controls have similar characteristics to treated firms at the start of the Event Quarter (*EQ*) and similar returns during *EQ*. In particular, our matching procedure consists of the following steps.

1. As of the beginning of *EQ*, we search for controls that have:
   (a) the same narrow credit rating as the treated firm,
   (b) the same Fama-French five industry classification, and
   (c) a propensity to be a fire-sale stock within 2.5% of that of the treated firm.
2. From these potential matches, we pick the firm that with the minimal absolute distance in stock return from the treated firm in *EQ*, excluding any matches with an absolute return difference of more than 2.5%.
3. If a satisfactory match cannot be established within a narrow rating category, we then look for a control candidate within a broader rating category (i.e., ignoring '+','-').

The propensity score caliper of 2.5% corresponds to one-third of the standard-deviation of treated firm propensity scores. The return caliper of 2.5% corresponds to one-fifth of the standard deviation of treated firm *EQ* returns. Our matching criteria are chosen to balance the need for a tight match and a large sample. We show in Section 2.3 that this procedure results in treated and control firm samples that are similar across a variety of dimensions, and in Section 4.5 that our main results are similar if we relax or tighten the criteria, or use a different matching procedure.

The matched sample analysis allows us to account for common shocks across treated and control firms. The control sample provides an estimate of the downgrade rate that we expect for firms with similar characteristics and a similar *EQ* return as treated firms. The key difference between the two samples is that the treated firm $EQ$-return is transitory. While we cannot rule out the possibility that some treated firms experience permanent shocks or that some controls experience transitory shocks, our setup ensures that the treated sample is more likely to experience transitory shocks. Moreover, we confirm in the data that on average, returns for the treatment firms reverse while those for the controls do not.

A causal interpretation of our results requires that the selection of stocks into the fire sale sample is independent (conditional on covariates) from the actions of CRAs. The argument for such independence is similar to the argument that Edmans, Goldstein and Jiang (2012) make for fire sales and takeover likelihood: decisions by investors to buy or sell a particular mutual fund are unlikely to be due to information about changes in credit ratings of specific stocks within the fund. Investors with such information are more likely to trade on the individual stock or bond rather than the fund. Nevertheless, our research methodology consists of several elements that are designed to address potential sources of endogeneity. First, as discussed above, we follow Edmans, Goldstein and Jiang (2012) in using implied rather than actual sales of mutual funds as the source of exogenous variation. Thus, our tests do not reflect discretionary trades that may be based on changes in fund manager views about the firm in the event quarter. Second, as in Edmans, Goldstein and Jiang (2012), we exclude sector funds to eliminate events where there may be specific information about the industry as a whole. Finally, we use propensity-score matching to ensure that there are no meaningful observable differences between treated and control firms prior to the fire-sale quarter.

In particular, we estimate a probability model for a firm to experience fire sales in a given quarter and match on the estimated propensity scores in the beginning of the event quarter. A fire-sale firm can differ from a typical firm for several reasons (Berger, 2015). First, because they are owned by certain mutual funds, they may have greater mutual fund ownership in general and also possess other characteristics associated with mutual fund ownership. We therefore include mutual fund ownership, size, leverage, liquidity, and volatility in our propensity score model. A second possible difference is that fire-sale stocks are in some way worse than the typical stock. This might be because fire-sale stocks are owned by fund managers that are losing assets under management—presumably because they have under-performed. We therefore include returns over the past three and past 12 months as well as rating changes over the past three and 12 months as additional predictor variables in the propensity score model. While we cannot rule out the possibility that treated firms are different from controls along some unobserved or mismeasured dimensions related to past performance, this possibility seems unlikely because, as we see below, past returns do not predict selection into the fire sales sample. Moreover, if fire-sale stocks are of worse quality than controls, this will bias us towards finding they are more likely to be downgraded than the controls.

*1.3. Measuring fire sales*

We closely follow the approach in Edmans, Goldstein and Jiang (2012) to construct $MFFlow$, the implied price pressure calculated by assuming that funds subject to large outflows (>5% of their assets) adjust their existing holdings in proportion to their previous portfolio weights. More precisely, we first calculate the dollar outflows of fund $j$ from the end of quarter $q-1$ to the end of quarter $q$ as follows:

$$Outflow_{j,q} = -(TNA_{j,q} - TNA_{j,q-1}(1 + r_{j,q})), \tag{1}$$

where $TNA_{j,q}$ is the assets under management of fund $j = 1,...,m$, in quarter $q$ and $r$ is the net return of fund $j$ in quarter $q$. In every quarter $q$, summing only over the $m$ funds for which the percentage outflow $(\frac{Outflow_{j,q}}{TNA_{j,q-1}})$ is greater than 5%, we then construct:

$$MFFlow_{i,q} = \sum_{j=1}^{m} \frac{Outflow_{j,q} * s_{i,j,q-1}}{\text{Volume}_{\text{i,q}}}, \tag{2}$$

where $i = 1,...,n$ indexes stocks, $Volume_{i,q}$ is the total dollar trading volume of stock during quarter $q$.

$$s_{i,j,q} = \frac{Shares_{i,j,q} * Price_{i,q}}{\text{TNA}_{\text{j,q}}}, \tag{3}$$

is fund $j$'s holdings of stock $i$ as a percentage of fund $j$'s TNA at the end of the quarter. Additional details regarding the construction of $MFFlow$ are in Appendix A.

Coval and Stafford (2007) and Edmans, Goldstein and Jiang (2012) define a fire sale as a firm-quarter where *MFFlow* falls below the 10th percentile value of the full sample. However, imposing a single threshold for the entire sample period affects the balance of the treated firm sample across time. In unreported tests, we find that using a single threshold for the full sample results in a large concentration of fire-sale firm-quarters during the Internet boom in 1999. To ensure that our results are not driven by a specific time period, we modify the full-sample 10% threshold. We define an event as a firm-quarter in which a firm's

*MFFlow* is in the top decile of all firms that quarter (the 'local cutoff'), *and* to ensure that these are indeed fire sales, we also require that it is in the top quintile of the full sample (the 'global cutoff').

Figure 1 plots cumulative average abnormal returns (CAARs) in the three quarters before and after fire sales for all fire-sale firms as well as the subsample of these firms that have credit ratings. In particular, the abnormal returns are measured relative to the CRSP equal-weighted index (Panel A), and also to characteristic-matched portfolios from Daniel et al. (1997) (DGTW, Panel B). Both panels show that abnormal returns for the full sample of stocks are significantly negative (-4% to -5%) during the event quarter. We do not observe significant negative abnormal returns prior to the event quarter. The figure is similar to that in Edmans, Goldstein and Jiang (2012), except that we find a slightly quicker recovery due to differences in sample periods and in the threshold imposed. The figure also shows that CAARs for the subsample of treated firms that have credit ratings appear muted relative to the full sample. Firms with credit ratings that experience fire sales have a smaller dip in prices in the event quarter and a faster recovery. These patterns are consistent with the fact that firms that have credit ratings are generally larger and more liquid than those without ratings and hence more resilient to price pressure from mutual fund fire sales. These patterns are also consistent with rating agencies successfully dampening down the effects of fire sales. Distinguishing between these alternatives is difficult and not crucial for our research question.

## 2. Setting up the tests

This section presents summary statistics, the propensity score model for a stock to be a fire sale, and the properties of treated stocks and matches.

### *2.1. Summary statistics*

Table 1 presents summary statistics for the sample used in this paper. Panel A shows the number of firm-quarters that are treated and not treated every year for the sample of firms that have credit ratings. The panel also reports the fraction of treated and non-treated firms that are downgraded every year. Overall, there are about 6,400 treated firm-quarters that are reasonably evenly distributed over time. Panel B displays summary statistics for other important variables used in our analysis including raw returns, risk-adjusted returns, CDS spread changes and firm characteristics such as (log) market capitalization, book-to-market equity, leverage, liquidity, and mutual fund ownership.

## *2.2. The Propensity score model*

Table 2 presents results for a propensity score model for a firm to be a fire-sale stock in quarter $q$. The predictor variables are as of the end of quarter $q$-1. We estimate both OLS and logit models with a dependent variable that equals one if a stock is a fire-sale stock that quarter. The first three specifications use OLS and also include time fixed effects (year-quarter). The first specification shows that small, illiquid stocks with low leverage and high mutual fund ownership are more likely to experience fire sales. The second specification also includes ratings changes over the past three months (i.e., quarter $q$-1) and the past 12 months ($q$-4 through $q$-1). The effects of past rating changes are, if anything, in the opposite direction from that predicted by the hypothesis that fire sales firms are of worse quality than a typical stock. An upgrade (rather than a downgrade) over the past 12 months increases the stock's likelihood to be a fire-sale stock. However, this effect disappears over the past three months (the sum of the past three- and past 12-month coefficient is close to zero). Specification 3 shows that past three-month and past 12-month returns do not predict the likelihood of downgrades. These results are not consistent with the hypothesis that some mutual funds experience fire sales because the stocks they hold have worse performance prior to the event quarter.

Column (4) in Table 2 is the specification that we use for propensity score matches in our tests. This is a conditional logit specification that allows for fixed effects in a panel setting. As in the earlier specifications, we have year-quarter fixed effects. The column reports marginal effects evaluated at mean values. Coefficients are similar in magnitude between the OLS and conditional logit specifications.

Figure 2 shows propensity scores for treated and control firms. To ensure comparability across time, we set the fixed effects to zero.[13] This figure suggests that there is reasonable overlap between treated firms and controls.

## *2.3. The matches*

Table 3 shows that our matching procedure, described in detail in Section 1.2, achieves reasonable covariate balance. Despite imposing stringent matching criteria, we are able to find matches for over two-thirds

[13] This implies that the levels of the propensity scores are not easily interpretable as a probability. Specifically, the mean probability of being a fire-sale stock in the figure is much higher than the true mean, because the intercepts that are set to zero, are negative. However, it ensures that the distance in probability between stocks is comparable across different periods.

of the treated sample. Panel A shows that treated and control firms have similar means and standard deviations for all variables in the propensity score model. In particular, means for size, leverage, mutual fund ownership, and past returns are not different between treated and control firms in economic or statistical terms. The Amihud ratio is statistically higher for treated firms than for controls. However the difference is economically small (about 0.005 or one-seventh of a standard deviation in the treatment sample) and unreported tests confirm that the Amihud ratio does not predict downgrades. There appears to be no difference in the average change in credit rating before the event quarter but a somewhat stronger tendency for treated firms to be downgraded over the 12 months before the event quarter than controls. This difference is not statistically significant at conventional levels and we confirm that there are no pre-$EQ$ trends in rating actions in our subsequent analysis.

Panel B shows a reasonable balance between treatment and control samples even for a set of variables not included in the propensity score model. CAPM $\beta$ and book-to-market are similar across treated and control samples. Consistent with our matching design, risk-adjusted event quarter returns are similar for treated and control firms at approximately -2%. The table shows that treated firm returns are transitory: they completely revert over the next nine months. However, control firm returns do not reverse at all over the next nine months. Panel B also reveals that these equity price shocks translate to changes in default probability. We measure default probability using the model estimated in Campbell, Hilscher and Szilagyi (2008). To interpret magnitudes of default probabilities, note that this model outputs a monthly probability of default for month $t$+12. Because treated and control firms are well-matched at the start of $EQ$ and have similar returns in $EQ$, changes in CHS default probability are similar for treated and control firms during $EQ$. Average default probabilities for treated (control) firms increase from 0.053% (0.055%) to 0.060% (0.062%) over the event quarter. While these magnitudes may appear modest, note that these are monthly probabilities and these increases correspond to a move from the 60th to 75th percentile for a typical year. Moreover, in unreported tests, we find that the increases in default probability are statistically significant at 5% confidence level. Treated firm default probabilities begin to reverse nine months after $EQ$, but those of control firms remain elevated, resulting in significant differences between treated and control firm default probabilities nine months after $EQ$. Thus, perhaps not surprisingly, the CHS model does not distinguish between permanent and transitory shocks to credit risk in real time. The differences between these shocks is only visible in the default probabilities nine months after the shock, when transitory equity returns have fully reversed.

## 3. Key Results

This section presents our key results on whether CRAs and CDS markets can discern transitory shocks to credit risk.

### *3.1. Can CRAs see through transitory shocks?*

Table 4 presents the main results of this paper. Panel A presents realized downgrade probabilities of treated and control firms over the four quarters *EQ*-2 through *EQ*+1, where *EQ* is the fire sale event quarter. Realized downgrade probability is the fraction of firms in the relevant sample (treated or control) that experience a downgrade over a given period. Over the three mont perdiod *EQ*-1 (six month period *EQ*-2 and *EQ*-1), treatment and control firms exhibit parallel trends with similar downgrade probabilities of 2.0% (3.9%) for treated firms and 2.0% (3.8%) for controls. During the event quarter, treated firms have a much lower downgrade probability (2.2%), than controls (3.1%). The difference of 92 basis points is highly significant statistically (heteroskedasticity robust t-statistic of -2.9).[14] The effect is present one quarter after *EQ* as well, with the difference in downgrade probabilities between treated (2.4%) and control firms (3.3%) of 89 basis points. Overall, for the six month period starting with *EQ*, the difference between treated and control firms is -1.5% (t-statistic of -3.43), two-fifth of the pre-*EQ* downgrade rate for treated (or control) firms.

Although the average realized downgrade probability for treated firms during *EQ* is not zero (2.2%), this does not necessarily show that CRAs have failed to discern some transitory shocks. Because treated firms are also subject to fundamental shocks, zero is not the relevant benchmark. The correct benchmark reflects the rate of arrival of fundamental shocks to a sample of firms that is similar to the treated sample and does not condition on contemporaneous returns. The downgrade probability for treated (or control) firms in the quarter prior to the event quarter meets this criteria. We see that downgrade probabilities for treated firms

[14]We follow Abadie and Imbens (2006) to compute standard errors using the conditional variance with up to 15 nearest neighbors.

are similar in $EQ$ and $EQ$-1 (0.020 and 0.022), suggesting that CRAs ignore the price pressure due to fire sales.

Next, we incorporate the severity of rating downgrades in our analysis. Panel B reports results of tests that use the number of notches downgraded as the dependent variable. This variable is zero for upgrades or if there is no change in the credit rating, and equals the number of notches downgraded if there is a downgrade over the test period. These results are similar to those for realized downgrade probabilities considered in Panel A. Treatment and control samples again display parallel trends before the event quarter, with similar expected downgrade notches over the six moths prior to the fire-sale quarter. Over the following two quarters treated firms have significantly lower expected downgrade notches as compared with controls (0.114 versus 0.163). This difference of 0.0486 downgrade notches is large relative to the average downgrade notches of 0.083 (0.085) over the six months before $EQ$ for treated (control) firms.

One possibility is that controls are just more volatile than treated firms during the event quarter with greater probabilities of upgrades and downgrades (although we explicitly match on pre-$EQ$ volatility and $EQ$ returns). To investigate this question, Panel C of table 4 shows that there are no differences between treated and controls in the upgrade notches in $EQ$ and the subsequent quarter. We do find a statistically significant difference in the upgrades for the control firms before $EQ$. However, the difference in expected upgrade notches during $EQ$-2 and $EQ$-1 is small economically (0.016 versus 0.049 for expected downgrade notches during $EQ$ and $EQ$+1 as per Panel B) and suggest that CRAs are actually relatively more positive about control firm creditworthiness pre-$EQ$. Also, because we match on the end of $EQ$-1 credit rating, this pre-$EQ$ difference implies that a few control firms were upgraded to their current rating more recently than treated firms. This makes the subsequent difference in downgrade rates even more surprising because, if anything, CRAs prefer not to reverse recent changes in ratings (Cantor and Mann, 2006).

The matched sample setup implies that any hypotheses that rely on features of ratings common across treated and control firms are unlikely to explain our results. For example, both treated and control firms are equally impacted by discreetness in rating categories, or if CRAs are slow in general to update ratings.

As reported in Table 3, both treated and control firms have negative average excess returns in the event quarter. However, on average these returns are relatively small in absolute magnitude (about -2%). The next panels test whether CRAs are able to discern which shocks are transitory when the negative shocks are large in magnitude and potentially have greater economic impact. To do so, we restrict the sample to firms with negative raw returns in the event quarter. For this subsample, average returns in the event quarter are -12% for both treated and control firms (untabulated).

Panels A2 and B2 show significant differences between treated and control firms in downgrade probability (7.3% versus 9.2%, Panel A2) and expected number of notches downgraded (0.195 versus 0.28, Panel B2) over the six month period ($EQ$ and $EQ$+1) in this subsample. For $EQ$ alone, the difference in downgrade probabilities is 133 bps. Thus, the treatment effect increases by a factor of 1.45 for the subsample with negative $EQ$ returns relative to the full sample.[15] Panel C2 shows that there are virtually no differences in the expected upgrade notches during or before the $EQ$ in this subsample. However, over $EQ$ and $EQ$+1, treated firms have slightly greater expected upgrade notches than control firms. Note that this difference arises from a greater post-$EQ$ decline in upgrade probabilities for control firms relative to treated firms, consistent with control firms receiving a large, permanent negative shock on average in this subsample.

### *3.2. Can CDS markets see through transitory shocks?*

An alternative explanation for our results is that CRAs learn which shocks are transitory from other markets, rather than through any independent analysis on their part. Table 5 examines this hypothesis, by testing whether CDS markets respond differently to transitory and permanent shocks. These tests restrict the sample to firms with traded Credit Default Swaps (CDS). This sample has only 592 treated firm-quarters (as opposed to 4260 in the main tests) over the period 2002-2015 and significantly larger firms (on average $12 billion in market capitalization as opposed to $3.7 billion).[16]

First, Panel A tests whether our results on CRAs hold in the subsample of firms with traded CDS contracts. In particular, we repeat the analysis of CRA downgrades for treated and control firms from Panel A of Table 4 for this subsample. Panel B repeats the analysis with CDS spread changes instead of rating downgrades as the outcome variable. Finally, Panel C examines changes in credit ratings implied by the

[15]In this negative return subsample, the downgrade rate is higher for the treated firms too. This could be because CRAs are less confident in these cases and/or because a higher fraction of treated firms actually experience adverse fundamental shocks along with the liquidity shock when we condition on negative returns.

[16]We use the same matching criteria as in the main analysis (section 2.3) but re-estimate the fire sale probability model from 2 on the CDS subsample and augment it with past changes in the CDS implied rating. The results are very similar (available upon request) if the overall sample propensity is used instead (or other covariates added) except for the implied rating change analysis in Panel C, where the implied rating downgrade probability is lower pre-$EQ$ (while not being different during or post $EQ$). Hence, we re-estimate the model over the CDS subsample to get a better match with parallel trends pre-event.

level of CDS spreads (rather than actual ratings by CRAs) as estimated by Markit, the CDS data provider. Since implied rating changes will reflect only substantial changes in the perceived creditworthiness, they could be viewed as more comparable to the CRA's actions.

Panel A shows that the difference in CRA downgrade probability between treated and control firms is large and statistically significant. The treatment effect is 2% for $EQ$ and 3.9% for the 6 months starting with $EQ$, which is a little larger than the effect in the main sample. Meanwhile, Panel B shows that CDS spreads increase by similar magnitudes for both treated and control firms (20 and 16 basis points respectively) over $EQ$ and $EQ$+1. There are no significant differences between treated and control firms CDS spreads either before or after $EQ$; if anything, treated firm CDS spreads tend to widen slightly more than controls over $EQ$ and $EQ$+1. Panel C shows that results are similar if implied ratings from CDS markets are used instead of CDS spreads. Taken together, these results show that unlike CRAs, CDS markets do not distinguish between transitory and permanent shocks in real time.

We also note that the CDS spreads appear to lag stock markets—a large part of the increase in spreads takes place during $EQ$+1 rather than in $EQ$. This lag is also visible in Figure 3, which presents stock returns (Panel A) along with cumulative CDS spread changes (Panel B) for the CDS sample over the three quarters around $EQ$. The figure computes cross-sectional means of the outcome variables in each quarter, followed by time-series means as in Coval and Stafford (2007). Hence, magnitudes are different from those in the table which presents full-sample means. The lagged response of CDS markets to stock returns is consistent with Hilscher, Pollet and Wilson (2015) who find that information appears to flow from equity to CDS markets.[17] Figure 3 also shows that increases in CDS spreads for treated firms are indeed transitory; spreads reverse back to their pre-$EQ$ levels during $EQ$+2. However, control firm CDS spreads remain elevated through the $EQ$+3, thereby confirming our research design. The question of whether CDS spreads *should* react to transitory equity price shocks is a thorny one. At one level the market value of equity has just fallen, thereby increasing leverage and hence default probability, so perhaps an increase in CDS spreads is warranted. But this increase is transitory and reverses over the next few quarters. If CDS market participants are aware that the shock was transitory, would spreads on five year CDS contracts increase? In any case,

[17] Chava, Ganduri and Ornthanalai (2016) find that equity market responses to credit rating downgrades are muted if the firm has CDS contracts traded on it. They argue that this is due to information flowing from CDS to equity markets prior to the downgrade (they do not find that information flows from CDS to equity markets "at times other than just prior to downgrades").

either due to differing objectives between CDS markets and CRAs, or because CRAs have information that CDS markets do not, there is a difference between the reaction of CRAs and CDS spreads to transitory price shocks in equity markets. If CRAs did not exist, and CDS spreads were used in contracts as measures of credit risk, these results suggest that transitory price shocks in equity markets would be reflected in CDS spreads and potentially propagate to the real economy.

## 4. Additional tests

In this section, we dig deeper to understand the cross-section and time series of the response of CRAs to fire-sale stocks. In the cross-section we test whether the treatment effects are strongest where we a priori expect them to be if fire-sale shocks are transient. Then we test whether the treatment effect is different across rating categories and over time. Finally, we examine a placebo test and subject our matching procedure to robustness tests.

### *4.1. The V-shaped pattern in returns and CRA actions*

The defining feature of a deep transitory shock is the V shape in returns: stock prices fall in the event quarter and recover over the next few quarters. A shallower dip or no subsequent recovery may be situations in which the economic impact of fire sales is small or where we have misclassified permanent shocks as temporary ones. If CRAs are indeed able to perceive transitory shocks, their actions should be most salient for shocks that most closely exhibit the V-shaped pattern in returns.

To test this hypothesis, we classify treated firms into two groups based on their returns in the event quarter. We also independently sort treated firms into two groups based on excess returns (over the market) in the six months after the event quarter.[18] We choose the six month horizon because on average, treated firm returns in our sample recover by the end of quarter $EQ$+2 (see Figure 1). We measure excess returns over the market for the recovery because any information that CRAs have is likely to be firm-specific and not systematic. Firms in the top right bin (low $EQ$ return, high return over the next two quarters) are firms with the most pronounced V-shape in returns, where we expect the greatest difference between downgrade

[18] We note that higher downgrade rates for control firms may by itself cause lower returns in the control group. We therefore explicitly avoid conditioning on the control group returns in this analysis.

probabilities of treatment and control firms. In contrast, in the bottom left bin (high *EQ* return, low next two quarter return difference), we are less confident that the shock is indeed transitory or even present. We expect smaller differences between treatment and control firms in this bin.

Panel A of Table 6 reports differences in downgrade probability. Firms with the most pronounced V-shape—those, in the low event quarter return and high post-*EQ* return group—have a lower likelihood of downgrades relative to controls by -1.44% while the difference in downgrade probability for the least V-shaped group is -0.57%. Panel B conducts similar analysis but with regards to downgrade notches while Panels A2 and B2 focus on the subsample with only negative *EQ* returns (as do Panels A2-B2 of Table 4). Across all panels we see a similar patterns: the difference in the most V-shaped group is 2.5-4 times larger than in the least V-shaped group; if there is strong recovery, the difference in downgrade probability becomes more negative as the event quarter return falls.

### *4.2. The effect across rating categories*

Figure 4 shows downgrade probabilities for treated and control firms by broad rating category. Realized downgrade probabilities continue to measure downgrades across narrow categories—we merely present results by broad rating categories (i.e., ignoring '+' and '-') in the figure to ensure sufficiently large samples within each rating category. Panels A and B show that in general across all firms, whether treated or control, downgrade probabilities follow a 'U'-shaped pattern. Downgrade probabilities decrease as credit risk increases from AA, reaching a minimum at BBB, and increasing thereafter.[19] BBB firms are just above the investment grade threshold.

Panel B shows that the treatment effect, which is the difference between downgrade probabilities of treated and matched control firms, is robust. In particular, treated firms are less likely to be downgraded than are controls for all categories except AA. The latter difference may be insignificant because we are able to match only 90 AA treated firm-quarters (untabulated), 3.5-times less than the next smallest bin (B).

We also do not see a difference in the treatment effect around the investment grade threshold (BBB to BB). As one moves from A to BBB ratings, both treated and control firms have lower downgrade probabilities. Moving from BBB to BB results in an increase in downgrade probabilities for both treated and control

[19] We do not plot AAA and categories below B because they have few observations.

firms. The treatment effect is present for both BBB and BB firms, but is not statistically different across the investment grade threshold.

A priori, it is not clear what difference in downgrade probabilities between treated and control firms at the investment grade threshold should be. One possible argument is that because the real effects of a downgrade are highest at this threshold, CRAs should be the most careful and therefore better distinguish between transitory and permanent shocks in situations when the firm's investment grade status is at stake. However, this argument (i) implies that CRAs apply insufficient effort to distill information from stock returns at other rating levels, and (ii) ignores greater incentives for all BBB-rated firms to take corrective actions, such as, asset sales and raising additional equity to retain their investment grade status.

### *4.3. The effect over time*

This section examines time-variation in the treatment effect. There are two motivations for examining this time-series. First, we want to be sure that the results are not driven by a few years, which might imply that they are due to specific events such as, the financial crisis. A second motivation is that subsamples based on time allow us to examine whether the ability of CRAs to see through transitory shocks is due to their access to non-public information from firm managers.

In particular, Regulation Fair Disclosure ('Reg FD') changed the access of CRAs to non-public information relative to the market when it was enacted in October 2000. CRAs were specifically exempt from the provisions of Reg FD, which applied to other market participants, such as equity analysts. Jorion, Liu and Shi (2005) find that the enactment of Reg FD affected the information content of downgrades for financial markets. Stock price reactions to downgrades are significantly greater in the period after Reg FD relative to just prior to its enactment.

The financial crisis and its aftermath have also affected the legal environment that CRAs operate in. In October 2010, the SEC removed the CRAs explicit exemption from Reg FD. It is not clear whether this change has had any material effect on the access of CRAs to non-public information. CRAs argue that they are not covered by Reg FD in any case because they meet other criteria for exemption: they do not seek to make investment decisions based on the private information, and their engagement letters with firms contain confidentiality agreements (Carbone, 2010). Nevertheless, the repeal of the exemption may have created ambiguity among firms, who may decide to play it safe and restrict access of CRAs to non-public information.

Besides removing the exemption from Reg FD, the Dodd-Frank Act also made other changes to the legal environment for CRAs. These include increasing the legal liability for issuing inaccurate ratings, and making it easier for the SEC to impose sanctions against CRAs. Dimitrov, Palia and Tang (2015) find that these changes diminished the information content of CRAs for equity and bond markets. Finally, public criticism of the role of CRAs in causing the financial crisis may have weakened the trust that markets have in credit ratings. This lack of trust may also diminish the incentives for firms to provide non-public information to CRAs after the crisis, if they believe that market participants will not pay attention to ratings.

Figure 5 shows downgrade probabilities for treated and control firms in the event quarter (Panel A) and in the event quarter and subsequent quarter (Panel B). We split the sample into pre and post the onset of the 2007–2009 financial crisis. We examine four subperiods: the period before Regulation FD (from 1991 until the last quarter of 2000), the original Reg FD period before the crisis (first quarter of 2001 to the second quarter of 2007), the crisis (third quarter of 2007 to first quarter 2009), and the postReg FD exemption removal period. The treatment effect is highest in the original Reg FD period when CRAs had privileged access to information and lowest during the crisis. The effect is positive, but small in the post-crisis, post-Reg FD period. However, we cannot draw strong conclusions from this analysis as subsample means are typically not significantly different from each other.

### *4.4. Placebo test*

This section reports a placebo test that examines whether our results are specific to fire sales by mutual funds, or are due to properties of mutual fund ownership and outflows. In the placebo test, our treatment sample is derived from all mutual fund sales that are not fire sales. In particular, the fire sale variable is constructed by assuming that mutual funds that experience outflows greater than 5% of their assets sell their holdings in proportion to their beginning-of-quarter weights. For the placebo test, we remove the 5% threshold and generate a placebo treatment variable based on sales of *all* mutual funds in proportion to their weights in response to outflows. To ensure that this is truly a placebo, we exclude any fire-sale stock-quarters from the placebo sample. Placebo-treated firms are those with total implied selling pressure in the top local and global quartile of mutual fund sales. Because we lose some firms when we exclude true fire sales, we use quartiles as the cutoff to ensure that sample sizes are comparable over placebo and actual treated samples.

Control firms are identified in the same manner as in our main results in Table 4.

Table 7 shows the results of this placebo test. We find no difference in the downgrade probabilities between the placebo-treated and control firms before, during, or after the placebo treatment quarter. These results show that the variation related to the mutual fund ownership and trading in general (e.g. mutual fund holdings, stock liquidity etc.) does not drive our results. In unreported results, we find that returns for the placebo treatment sample do not reverse after the placebo treatment quarter, confirming that these are not transitory shocks.

*4.5. Robustness tests*

As discussed above, our matching procedure balances the need to maximize sample size with the need to have close matches. Table 8 examines the robustness of our results to changing our matching criteria. Columns (1) through (5) are identical to those in our main specification in Table 4. For brevity, we focus on differences in downgrade probabilities between treated and control firms over the six-month interval starting with $EQ$.

The first line in Table 8 reproduces the baseline results from Table 4 for ease of comparison. We consider several changes to the matching procedure and to the data sample. Within each major robustness category, we report results where we increase the maximal event quarter return distance between a treated and control firm ('+ wider return caliper'), maximal pre-$EQ$ propensity score distance between treated and control firms ('+ wider pscore caliper'), and number of controls matched for each treated firm ('+ multiple controls'). The $+$ sign denotes that the current specification is the previous specification along with the change specified after the $+$ sign.

In the first set of results, we see that increasing the return caliper, increasing the propensity score caliper, and adding additional controls per treated firm do not significantly affect the baseline results. These changes serve to increase the sample size by approximately a 1000 firms.

The next specification examines the robustness of the main result to excluding the period of financial crises of 2007–2009, firms in finance-related industries and utilities, and dropping the biggest part of the sample—manufacturing firms. We see that the difference between treated and control groups is statistically significant across these subsamples, and is typically within one standard deviation of our baseline estimations for all firms and for those with established CDS markets.

Next, we change some of the criteria used in the baseline matching scheme. First, we examine the effects of matching only within narrow rating categories (i.e., BBB is now considered different from BBB−). In our main specifications we first search for a match within the narrow category, then seek a match across the broad category if no matches are available within the narrow category. Restricting matches to within narrow categories reduces the sample size by approximately 1000 firms. However, the treatment effect remains robust and about the same magnitude as the baseline. Increasing sample size by relaxing return and propensity score calipers within the narrow matching scheme does not affect results.

Next, we repeat the analysis using a finer industry classifications scheme, the Fama-French 12 industry classification, instead of the five industry classification used in the baseline. We then consider a match without regards to industry. Neither change materially affects the significance and magnitude of the treatment effect.

We also consider a different matching scheme. Rather than matching treated firms to controls that have the closest $EQ$ return within a propensity score caliper, we match them to controls with the closest propensity score (subject to a maximum difference of 0.025) and $EQ$ returns within the same quintile (or decile). We do this for both the narrow rating matches as well as coarse rating matches. Results are robust to all these changes.

Additionally, in the Appendix (Tables A4 and A5 ), we examine whether results are similar if we use Moody's ratings instead of S&P. Our Moody's sample is shorter (1990–2008) and has a lower match rate to CRSP data, leading to substantially fewer observations. Nevertheless, the difference between treated and control firms in downgrade probability, as well as the number of notches downgraded over $EQ$ and $EQ$+1 is significant and of similar magnitude to the larger S&P sample.

Finally, a potential hypothesis is that CRAs wait to see if a shock begins to reverse within a quarter and only downgrade firms if there is no reversal. However, this hypothesis is inconsistent with the data, because it implies that CRAs would not downgrade control firms since these firms have permanent shocks to returns. Nevertheless, in the Appendix, we restrict the sample of treated firms to those with no meaningful recovery within $EQ$ and find similar results.

Overall, these results suggest that our baseline estimations are representative of the effect of fire sales on rating downgrades and that our statistical inference is robust to the matching scheme choice and peer-firm definition.[20]

## 5. Conclusion

This paper shows that CRAs distinguish between transitory and permanent shocks to credit risk, while CDS markets do not. Our paper has three related implications. First, our results imply that CRAs actually play a role as information intermediaries. One of the traditional arguments for the existence of CRAs is that they act as intermediaries between borrowers and the market. Rather than revealing potentially private information to the entire market (including competitors), firms can reveal information to CRAs who analyze the information and provide a public summary of the information that markets are interested in: is the borrower still creditworthy? However, given the availability of market-implied measures of credit risk for publicly-traded firms and concerns regarding the accuracy of CRAs due to conflicts of interest and catering, it is not clear what value CRAs add as information intermediaries. We demonstrate one channel through which CRAs add value: they are able to distinguish between transitory and permanent shocks to credit risk.

A related implication is that markets are not perfect substitutes for CRAs. For example, Flannery, Houston and Partnoy (2010) argue that CDS spreads should be used instead of credit ratings in contracts and regulations. Our results suggest that if measures of credit risk based on market prices are embedded into contracts or used for regulatory purposes, it might allow transitory shocks in financial markets to propagate to the real economy. For example, a transitory shock to credit risk could trigger a contractual provision across all of a firm's suppliers and thereby affect the firm's ability to purchase raw materials. This will in turn affect the firm's production and could cause additional real effects downstream. Thus, our results suggest that CRAs may act as circuit-breakers by dampening the real effects of friction-driven shocks in equity markets.

This role of CRAs depends crucially on their access to private information and their ability to process this information. Since the financial crisis, CRAs have lost some credibility with regulators and markets and the thrust of regulatory policy over the past few years has been to reduce the special role of CRAs. For example, the Dodd-Frank act mandates the removal of ratings from regulations and also removes the

[20] Additional unreported tests show that inferences stay the same for similar robustness tests on the negative $EQ$-return and CDS subsamples.

exemption of CRAs from Reg FD. A final implication of our results is that although regulations that reduce the access of CRAs to private information may have benefits (e.g. encouraging information production from other market participants instead of relying on CRAs), such regulations also have costs. Specifically, if CRAs do not have access to information, they may not be able to distinguish between real and transitory shocks to market prices. This could amplify the real effects of transitory shocks to market prices.

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## **Table 1:** Summary statistics

This table reports summary statistics for the data used in this study. Panel A reports the number of firm-quarters of treated and not treated observations each year in our sample. Treated firms are those that experience fire sales by mutual funds as defined in Section 1.3. Panel B provides summary statistics for other variables used in our study at the firm-quarter frequency.

**Panel A:** The rated firm-quarter sample

| | Treated | | | Not Treated | | |
|---|---|---|---|---|---|---|
| | Firm-Qtrs | Downgrades | Ratio | Firm-Qtrs | Downgrades | Ratio |
| 1990 | 70 | 6 | 0.086 | 1,873 | 116 | 0.062 |
| 1991 | 110 | 2 | 0.018 | 2,362 | 109 | 0.046 |
| 1992 | 84 | 0 | 0.000 | 2,397 | 67 | 0.028 |
| 1993 | 87 | 1 | 0.011 | 2,919 | 92 | 0.032 |
| 1994 | 219 | 1 | 0.005 | 3,377 | 83 | 0.025 |
| 1995 | 185 | 4 | 0.022 | 3,558 | 89 | 0.025 |
| 1996 | 236 | 6 | 0.025 | 3,853 | 81 | 0.021 |
| 1997 | 276 | 4 | 0.014 | 4,063 | 95 | 0.023 |
| 1998 | 318 | 7 | 0.022 | 4,829 | 171 | 0.035 |
| 1999 | 390 | 6 | 0.015 | 5,296 | 220 | 0.042 |
| 2000 | 475 | 18 | 0.038 | 4,970 | 236 | 0.047 |
| 2001 | 174 | 8 | 0.046 | 4,647 | 266 | 0.057 |
| 2002 | 359 | 18 | 0.050 | 4,909 | 325 | 0.066 |
| 2003 | 302 | 4 | 0.013 | 4,757 | 216 | 0.045 |
| 2004 | 369 | 4 | 0.011 | 4,983 | 143 | 0.029 |
| 2005 | 313 | 12 | 0.038 | 5,018 | 187 | 0.037 |
| 2006 | 329 | 7 | 0.021 | 5,177 | 179 | 0.035 |
| 2007 | 274 | 4 | 0.015 | 5,178 | 204 | 0.039 |
| 2008 | 238 | 24 | 0.101 | 5,103 | 304 | 0.060 |
| 2009 | 221 | 21 | 0.095 | 4,694 | 320 | 0.068 |
| 2010 | 264 | 3 | 0.011 | 4,699 | 99 | 0.021 |
| 2011 | 186 | 4 | 0.022 | 4,899 | 132 | 0.027 |
| 2012 | 325 | 6 | 0.018 | 4,786 | 137 | 0.029 |
| 2013 | 241 | 1 | 0.004 | 4,840 | 90 | 0.019 |
| 2014 | 218 | 3 | 0.014 | 5,073 | 92 | 0.018 |
| 2015 | 101 | 2 | 0.020 | 2,607 | 90 | 0.035 |
| Total | 6,364 | 176 | 0.028 | 110,867 | 4,143 | 0.037 |

**Panel B:** Other variables

| | Firm-Qtrs | mean | sd | skew | p1 | p25 | p50 | p75 | p99 |
|---|---|---|---|---|---|---|---|---|---|
| Return (Raw) | 143,376 | 0.029 | 0.211 | 0.275 | -0.569 | -0.079 | 0.028 | 0.133 | 0.758 |
| CAPM $\beta$ | 137,108 | 1.094 | 0.710 | 1.178 | -0.113 | 0.610 | 0.996 | 1.440 | 3.375 |
| Return (DGTW) | 105,518 | -0.001 | 0.176 | 0.334 | -0.507 | -0.094 | -0.006 | 0.084 | 0.609 |
| log(Realized Variance) | 143,657 | -7.714 | 1.122 | 0.498 | -9.876 | -8.502 | -7.809 | -7.042 | -4.628 |
| log(Mkt CAP) | 143,646 | 7.396 | 1.859 | -0.260 | 2.573 | 6.263 | 7.458 | 8.599 | 11.576 |
| Book-to-Market | 128,119 | 0.786 | 1.221 | 7.054 | 0.013 | 0.283 | 0.549 | 0.910 | 5.779 |
| Debt-to-EV | 128,157 | 0.282 | 0.178 | 0.343 | -0.000 | 0.146 | 0.282 | 0.404 | 0.662 |
| Mutual Fund Ownership | 139,960 | 0.141 | 0.110 | 1.137 | 0.001 | 0.051 | 0.119 | 0.212 | 0.436 |
| Amihud Ratio | 143,646 | 0.023 | 0.052 | 2.906 | 0.000 | 0.000 | 0.002 | 0.012 | 0.244 |
| Rating Change past 12 months | 142,298 | -0.215 | 1.606 | -6.969 | -5.000 | 0.000 | 0.000 | 0.000 | 2.000 |
| MFFlow | 143,657 | -0.007 | 0.097 | -300.1 | -0.073 | -0.006 | -0.002 | -0.000 | 0.000 |
| CHS Default Prob (%) | 109,816 | 0.085 | 0.201 | 10.37 | 0.015 | 0.029 | 0.042 | 0.067 | 0.944 |
| CDS spread change (bps) | 24,048 | 0.107 | 3.494 | 29.65 | -3.145 | -0.130 | -0.007 | 0.085 | 4.889 |

## **Table 2:** A probability model for fire sales

We estimate models for the probability of a stock to experience a fire sale as a function of one-quarter lagged firm characteristics, past rating changes, stock returns, and year-quarter fixed effects. The outcome is one if the firm-quarter meets the criteria to be a fire sale as defined in Section 1.3, and zero otherwise. Specification (1) through (3) report linear probability model estimates. Specification (4) reports marginal effects estimated at means from a conditional logit model. Standard errors for t-statistics reported in parentheses are clustered at firm level, */**/*** denote significance at 10/5/1% confidence level.

| | OLS | | | Logit |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log(Market Cap) | −0.0111*** | −0.0111*** | −0.0111*** | −0.0219*** |
| | (−8.97) | (−8.94) | (−8.92) | (−22.93) |
| log(1+ Debt-to-EV) | −0.0421*** | −0.0410*** | −0.0415*** | −0.0669*** |
| | (−4.31) | (−4.15) | (−4.14) | (−10.18) |
| MF Ownership | 0.2746*** | 0.2742*** | 0.2744*** | 0.3712*** |
| | (12.67) | (12.59) | (12.59) | (31.37) |
| Amihud Ratio | 1.0441*** | 1.0443*** | 1.0422*** | 0.8550*** |
| | (14.60) | (14.59) | (14.50) | (30.38) |
| log(Realized Variance) | −0.0260*** | −0.0257*** | −0.0257*** | −0.0389*** |
| | (−15.53) | (−15.24) | (−15.23) | (−29.98) |
| Rating Change (3 month) | | −0.0280** | −0.0293** | −0.0710*** |
| | | (−2.15) | (−2.24) | (−2.81) |
| Rating Change (12 month) | | 0.0383*** | 0.0401*** | 0.0988*** |
| | | (2.97) | (3.12) | (4.42) |
| Return (3 month) | | | 0.0019 | 0.0095 |
| | | | (0.53) | (1.38) |
| Return (12 month) | | | −0.0023 | −0.0026 |
| | | | (−1.17) | (−0.82) |
| Year-Quarter Effects | Yes | Yes | Yes | Yes |
| Observations | 111,116 | 110,278 | 110,277 | 110,277 |
| $R^2$ / Pseudo $R^2$ | 0.0371 | 0.0372 | 0.0372 | 0.0771 |

**Table 3:** Covariate balance for treated firms and controls

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of $EQ$), and return in $EQ$. See Section 1.2 for details. The fewer number of control relative to treatment firm-quarters indicates that some controls are matched to multiple treated firms. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest.

**Panel A:** Propensity-score contributors

| | N(Treated)=4260, N(Control)=4023 | | | | |
|---|---|---|---|---|---|
| | Means | | | Std Deviations | |
| | Treated | Control | P-value | Treated | Control |
| | (1) | (2) | (3) | (4) | (5) |
| MCap(USD bln) | 3.745 | 3.903 | 0.451 | 8.244 | 7.437 |
| Debt-to-EV | 0.318 | 0.322 | 0.682 | 0.210 | 0.209 |
| Mutual Fund Ownership | 0.191 | 0.184 | 0.223 | 0.119 | 0.114 |
| Rating Change past 3 months | -0.008 | 0.000 | 0.489 | 0.415 | 0.471 |
| Rating Change past 12 months | -0.033 | -0.003 | 0.129 | 0.777 | 0.915 |
| Return past 3 months | 0.033 | 0.035 | 0.541 | 0.155 | 0.165 |
| Return past 12 months | 0.150 | 0.146 | 0.759 | 0.338 | 0.358 |
| Volatility past 3 month | 0.060 | 0.059 | 0.336 | 0.034 | 0.036 |
| Amihud Ratio | 0.019 | 0.013 | 0.000 | 0.040 | 0.034 |

**Panel B:** Other variables of interest

| | | Means | | | Std Deviations | |
|---|---|---|---|---|---|---|
| | | Treated | Control | P-value | Treated | Control |
| | | (1) | (2) | (3) | (4) | (5) |
| pre EQ | CAPM $\beta$ | 0.937 | 0.968 | 0.393 | 0.604 | 0.663 |
| | Book-to-Market | 0.728 | 0.722 | 0.820 | 0.855 | 0.896 |
| | Book leverage | 0.386 | 0.403 | 0.274 | 0.279 | 0.273 |
| | CHS Default Probability | 0.053 | 0.055 | 0.308 | 0.060 | 0.073 |
| During EQ | Raw Return | 0.013 | 0.012 | 0.987 | 0.147 | 0.147 |
| | Excess Return (Mkt) | -0.018 | -0.018 | 0.988 | 0.129 | 0.128 |
| | Excess Return (DGTW) | -0.015 | -0.019 | 0.241 | 0.121 | 0.123 |
| | CHS Default Probability | 0.060 | 0.062 | 0.253 | 0.101 | 0.104 |
| 9m after EQ | Cumulative Return | 0.143 | 0.096 | 0.004 | 0.343 | 0.327 |
| | Cumulative Return (vs Mkt) | 0.070 | 0.025 | 0.007 | 0.316 | 0.300 |
| | Cumulative Return (vs DGTW) | 0.041 | 0.001 | 0.003 | 0.267 | 0.266 |
| | CHS Default Probability | 0.058 | 0.063 | 0.069 | 0.093 | 0.101 |

## **Table 4:** Fire sales and credit ratings

This table examines the effect of mutual fund fire sales on credit ratings. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of *EQ*), and return in *EQ*. See Section 1.2 for details. Column (3) presents 'Average Treatment effect on Treated' (ATT), or the difference in the outcome variable between treated and control firms; a negative number indicates lower mean outcomes for treated firms relative to controls. In Panel A, the outcome variable equals 1 if the credit rating was downgraded during the period and zero otherwise. In Panel B, we take into account the severity of downgrades by reporting the average number of notches downgraded ($E[\#NotchesDown]$). Panel C reports the average number of notches upgraded. Panels A2, B2, and C2 report the same tests as panels A, B, and C but on a subsample of firms with negative *EQ* returns. The time periods we consider are: *EQ*-2 and *EQ*-1 (the 6 months before the start of *EQ*), *EQ*-1, *EQ*, *EQ*+1, and *EQ* and *EQ*+1 (the 6 months starting at the beginning of *EQ*). Standard errors and t-statistics reported in columns (4) and (5) respectively are robust for heteroskedasticity.

**Panel A:** $Pr\{Downgrade\}$

| | N(Treated) = 4260 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.039 | 0.038 | 0.0014 | 0.0039 | 0.36 |
| EQ-1 | 0.020 | 0.020 | -0.0002 | 0.0029 | -0.08 |
| Event Quarter | 0.022 | 0.031 | -0.0092 | 0.0032 | -2.87 |
| EQ+1 | 0.024 | 0.033 | -0.0089 | 0.0034 | -2.64 |
| EQ and EQ+1 | 0.045 | 0.060 | -0.0153 | 0.0044 | -3.43 |

**Panel B:** $E[\#NotchesDown]$

| | N(Treated) = 4260 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.085 | 0.083 | 0.0021 | 0.0096 | 0.22 |
| EQ-1 | 0.042 | 0.043 | -0.0007 | 0.0065 | -0.11 |
| Event Quarter | 0.050 | 0.070 | -0.0204 | 0.0076 | -2.70 |
| EQ+1 | 0.066 | 0.097 | -0.0310 | 0.0146 | -2.12 |
| EQ and EQ+1 | 0.114 | 0.163 | -0.0486 | 0.0171 | -2.84 |

**Panel C:** $E[\#NotchesUp]$

| | N(Treated) = 4258 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.068 | 0.084 | -0.0160 | 0.0080 | -2.00 |
| EQ-1 | 0.031 | 0.038 | -0.0075 | 0.0040 | -1.89 |
| Event Quarter | 0.029 | 0.030 | -0.0014 | 0.0036 | -0.39 |
| EQ+1 | 0.031 | 0.029 | 0.0026 | 0.0040 | 0.65 |
| EQ and EQ+1 | 0.060 | 0.059 | 0.0012 | 0.0052 | 0.23 |

**Table 4:** Fire sales and credit ratings (*Continued*)

**Panel A2:** $Pr\{Downgrade|EQ\,ret < 0\}$

| | N(Treated) = 2103 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.050 | 0.045 | 0.0052 | 0.0064 | 0.81 |
| EQ-1 | 0.024 | 0.028 | -0.0033 | 0.0047 | -0.71 |
| Event Quarter | 0.036 | 0.049 | -0.0133 | 0.0059 | -2.26 |
| EQ+1 | 0.041 | 0.053 | -0.0119 | 0.0063 | -1.87 |
| EQ and EQ+1 | 0.073 | 0.092 | -0.0195 | 0.0081 | -2.40 |

**Panel B2:** $E[\#NotchesDown|EQ\,ret < 0]$

| | N(Treated) = 2103 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.114 | 0.102 | 0.0114 | 0.0153 | 0.74 |
| EQ-1 | 0.056 | 0.060 | -0.0038 | 0.0107 | -0.36 |
| Event Quarter | 0.082 | 0.112 | -0.0300 | 0.0145 | -2.07 |
| EQ+1 | 0.117 | 0.177 | -0.0594 | 0.0292 | -2.03 |
| EQ and EQ+1 | 0.195 | 0.280 | -0.0842 | 0.0342 | -2.46 |

**Panel C2:** $E[\#NotchesUp|EQ\,ret < 0]$

| | N(Treated) = 2103 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.074 | 0.079 | -0.0057 | 0.0097 | -0.59 |
| EQ-1 | 0.037 | 0.038 | -0.0010 | 0.0065 | -0.15 |
| Event Quarter | 0.029 | 0.025 | 0.0048 | 0.0049 | 0.96 |
| EQ+1 | 0.028 | 0.018 | 0.0095 | 0.0050 | 1.88 |
| EQ and EQ+1 | 0.057 | 0.043 | 0.0138 | 0.0070 | 1.97 |

**Table 5:** Transitory fire-sale effects: Credit Ratings Agencies versus CDS markets

This table examines the effect of mutual fund fire sales on credit ratings and CDS spreads. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of $EQ$), and return in $EQ$. See Section 3.2 for details. Panel A reports the probability of downgrades during different periods (6 months before the $EQ$, 3 months before the $EQ$, the $EQ$, three months after the end of $EQ$, and 6 months starting with the beginning of EQ) for treated and control firms. Column (3) presents 'Average Treatment effect on Treated' (ATT), or the difference in realized downgrade probabilities between treated and control firms; a negative number indicates lower downgrade probabilities for treated firms relative to controls. Heteroskedasticity robust standard errors and t-statistics for the ATT statistic are reported in columns (4) and (5). Panel B conducts similar analysis but with changes in the Credit Default Swap spreads over the respective periods instead of downgrades as the outcome variable. Panel C repeats the analysis for ratings implied by the CDS spread levels (as reported by Markit) instead of ratings issued by CRAs. Due to the CDS data availability, the sample is constrained to 2002-2015 only.

| | N(Treated) = 592 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A:** Downgrades by CRA | | | | | |
| EQ-2 and EQ-1 | 0.042 | 0.029 | 0.0135 | 0.0100 | 1.35 |
| EQ-1 | 0.019 | 0.014 | 0.0051 | 0.0068 | 0.74 |
| Event Quarter | 0.017 | 0.037 | -0.0203 | 0.0075 | -2.69 |
| EQ and EQ+1 | 0.029 | 0.068 | -0.0389 | 0.0099 | -3.94 |
| **Panel B:** CDS Spread changes (basis points) | | | | | |
| EQ-2 and EQ-1 | 0.92 | -6.67 | 7.553 | 7.486 | 1.01 |
| EQ-1 | -0.36 | -3.58 | 3.221 | 5.503 | 0.59 |
| Event Quarter | 5.40 | 2.99 | 2.418 | 9.228 | 0.26 |
| EQ and EQ+1 | 20.28 | 15.83 | 4.456 | 12.588 | 0.35 |
| **Panel C:** Implied Downgrades by CDS | | | | | |
| EQ-2 and EQ-1 | 0.032 | 0.032 | 0.0000 | 0.0090 | 0.00 |
| EQ-1 | 0.012 | 0.015 | -0.0034 | 0.0048 | -0.70 |
| Event Quarter | 0.017 | 0.017 | 0.0000 | 0.0064 | 0.00 |
| EQ and EQ+1 | 0.037 | 0.035 | 0.0017 | 0.0105 | 0.16 |

**Table 6:** Fire-sale effects by return group

This table examines the effect of mutual fund fire sales on credit ratings across different groups of firms. Treated firms are sorted into four groups based on returns in the Event Quarter (*EQ*) and on returns in excess of the market over the 6 months after *EQ*. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of *EQ*), and return in *EQ*. We report 'Average Treatment effect on Treated' (ATT) for each group during *EQ*. In Panel A (B), ATT is the difference in downgrade probability (expected downgrade notches) between treated and control firms. Panels A2 and B2 restrict the sample to firms with negative *EQ* returns.

**Panel A:** ATT for $Pr\{Downgrade\}$

| | | Excess Return 6 months after EQ | | |
|---|---|---|---|---|
| | | Low | High | All |
| Return in the Event Quarter | Low | -0.0056 | -0.0144 | -0.0093 |
| | High | -0.0057 | -0.0116 | -0.0090 |
| | All | -0.0057 | -0.0130 | -0.0092 |

**Panel B:** ATT for $E[\#NotchesDown]$

| | | Excess Return 6 months after EQ | | |
|---|---|---|---|---|
| | | Low | High | All |
| Return in the Event Quarter | Low | -0.0094 | -0.0339 | -0.0191 |
| | High | -0.0115 | -0.0302 | -0.0219 |
| | All | -0.0103 | -0.0321 | -0.0204 |

**Panel A2:** ATT for $Pr\{Downgrade|EQret < 0\}$

| | | Excess Return 6 months after EQ | | |
|---|---|---|---|---|
| | | Low | High | All |
| Return in the Event Quarter | Low | -0.0090 | -0.0320 | -0.0190 |
| | High | -0.0074 | -0.0098 | -0.0087 |
| | All | -0.0081 | -0.0198 | -0.0133 |

**Panel B2:** ATT for $E[\#NotchesDown|EQret < 0]$

| | | Excess Return 6 months after EQ | | |
|---|---|---|---|---|
| | | Low | High | All |
| Return in the Event Quarter | Low | -0.0158 | -0.0800 | -0.0422 |
| | High | -0.0148 | -0.0246 | -0.0199 |
| | All | -0.0153 | -0.0495 | -0.0300 |

## **Table 7:** Placebo selling pressure and credit ratings

This table presents a placebo test for the main result of this paper. To identify placebo selling pressure, we reconstruct the treatment variable, $MFFlow$, using all funds that experience outflows instead of just those whose outflows are greater than 5% as we do in our main tests. We exclude any fire sale stock-quarters from the placebo sample (see Section 4.4 for details). We then replicate the analysis in Table 4 for this placebo treatment.

**Panel A:** $Pr\{Downgrade\}$

| | N(Treated) = 4092 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.053 | 0.052 | 0.0007 | 0.0046 | 0.16 |
| EQ-1 | 0.029 | 0.025 | 0.0034 | 0.0034 | 1.00 |
| Event Quarter | 0.032 | 0.035 | -0.0032 | 0.0036 | -0.88 |
| EQ+1 | 0.037 | 0.040 | -0.0027 | 0.0039 | -0.69 |
| EQ and EQ+1 | 0.064 | 0.069 | -0.0054 | 0.0049 | -1.09 |

**Panel B:** $E[\#NotchesDown]$

| | N(Treated) = 4092 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.128 | 0.124 | 0.0039 | 0.0120 | 0.33 |
| EQ-1 | 0.070 | 0.061 | 0.0090 | 0.0084 | 1.07 |
| Event Quarter | 0.075 | 0.084 | -0.0093 | 0.0094 | -0.99 |
| EQ+1 | 0.115 | 0.102 | 0.0130 | 0.0188 | 0.69 |
| EQ and EQ+1 | 0.184 | 0.181 | 0.0034 | 0.0218 | 0.16 |

**Panel C:** $E[\#NotchesUp]$

| | N(Treated) = 4083 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.060 | 0.057 | 0.0027 | 0.0057 | 0.47 |
| EQ-1 | 0.030 | 0.029 | 0.0012 | 0.0041 | 0.30 |
| Event Quarter | 0.030 | 0.026 | 0.0044 | 0.0037 | 1.19 |
| EQ+1 | 0.026 | 0.029 | -0.0029 | 0.0041 | -0.72 |
| EQ and EQ+1 | 0.056 | 0.055 | 0.0012 | 0.0054 | 0.22 |

## **Table 8:** Robustness tests

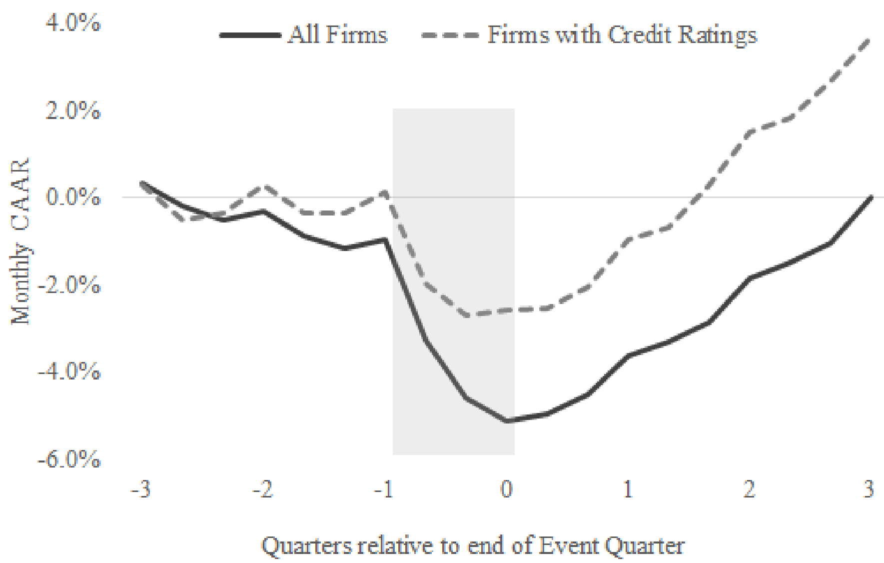
This table examines the robustness of the main results reported in Table 4 to different matching criteria and sample composition. Each row reports the difference in the realized probability of downgrades between treated and control firms during the 6 months starting at the beginning of the event quarter.

| | Industries cut | EQ Return caliper | Pscore caliper | Controls per 1 Treated | N(Treated) | Treated | Control | ATT | SE | t-stat |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | (1) | (2) | (3) | (4) | (5) |
| Baseline (Table 4 panel A) | 5 | 0.025 | 0.025 | 1 | 4,260 | 0.045 | 0.060 | -0.0153 | 0.0044 | -3.43 |
| + wider return caliper | 5 | 0.050 | 0.025 | 1 | 4,949 | 0.047 | 0.065 | -0.0184 | 0.0041 | -4.46 |
| + wider pscore caliper | 5 | 0.050 | 0.050 | 1 | 5,383 | 0.048 | 0.064 | -0.0152 | 0.0037 | -4.15 |
| + multiple controls | 5 | 0.050 | 0.050 | 2 | 5,383 | 0.048 | 0.064 | -0.0156 | 0.0037 | -4.25 |
| *Subsamples* | | | | | | | | | | |
| Exclude fin. crisis (3Q'07-1Q'09) | 5 | 0.025 | 0.025 | 1 | 3,975 | 0.036 | 0.055 | -0.0181 | 0.0038 | -4.13 |
| Exclude "Money"&"Util" (FF12) | 5 | 0.025 | 0.025 | 1 | 2,701 | 0.050 | 0.068 | -0.0178 | 0.0048 | -2.99 |
| Exclude "Manufacturing" (FF5) | 5 | 0.025 | 0.025 | 1 | 2,836 | 0.045 | 0.056 | -0.0109 | 0.0045 | -2.06 |
| *Different criteria within the baseline matching scheme* | | | | | | | | | | |
| Exact rating only match | 5 | 0.025 | 0.025 | 1 | 3,017 | 0.040 | 0.055 | -0.0149 | 0.0041 | -2.71 |
| + wider return caliper | 5 | 0.050 | 0.025 | 1 | 3,954 | 0.041 | 0.058 | -0.0175 | 0.0041 | -3.62 |
| + wider pscore caliper | 5 | 0.050 | 0.050 | 1 | 4,620 | 0.044 | 0.057 | -0.0128 | 0.0042 | -3.04 |
| + multiple controls | 5 | 0.050 | 0.050 | 2 | 4,620 | 0.044 | 0.058 | -0.0142 | 0.0038 | -3.38 |
| Finer industry match | 12 | 0.025 | 0.025 | 1 | 3,362 | 0.044 | 0.054 | -0.0107 | 0.0039 | -2.12 |
| + wider return caliper | 12 | 0.050 | 0.025 | 1 | 4,221 | 0.045 | 0.059 | -0.0135 | 0.0045 | -3.00 |
| + wider pscore caliper | 12 | 0.050 | 0.050 | 1 | 4,840 | 0.045 | 0.061 | -0.0159 | 0.0041 | -3.86 |
| + multiple Controls | 12 | 0.050 | 0.050 | 2 | 4,840 | 0.045 | 0.060 | -0.0154 | 0.0041 | -3.74 |
| Match without regards to industry | 0 | 0.025 | 0.025 | 1 | 5,314 | 0.049 | 0.065 | -0.0154 | 0.0036 | -4.30 |
| + tighter return caliper | 0 | 0.010 | 0.025 | 1 | 4,561 | 0.045 | 0.057 | -0.0118 | 0.0039 | -3.06 |
| + tighter pscore caliper | 0 | 0.010 | 0.010 | 1 | 3,747 | 0.042 | 0.052 | -0.0091 | 0.0047 | -1.94 |
| + multiple controls | 0 | 0.010 | 0.010 | 2 | 3,747 | 0.042 | 0.055 | -0.0121 | 0.0047 | -2.60 |
| *Different matching scheme – based on EQ return quantile and closest pscore* | | | | | | | | | | |
| Exact rating | 5 | 5 | 0.025 | 1 | 3,719 | 0.039 | 0.052 | -0.0134 | 0.0052 | -2.57 |
| + fine return grid | 5 | 10 | 0.025 | 1 | 3,272 | 0.037 | 0.060 | -0.0229 | 0.0058 | -3.96 |
| + fine return & industry grids | 12 | 10 | 0.025 | 5 | 2,300 | 0.039 | 0.054 | -0.0157 | 0.0063 | -2.49 |
| Coarse Rating | 5 | 5 | 0.025 | 1 | 4,488 | 0.045 | 0.057 | -0.0127 | 0.0051 | -2.51 |
| + fine return grid | 5 | 10 | 0.025 | 1 | 4,388 | 0.045 | 0.059 | -0.0144 | 0.0054 | -2.67 |
| + fine return & industry grids | 12 | 10 | 0.025 | 5 | 3,605 | 0.044 | 0.065 | -0.0214 | 0.0051 | -4.16 |

**Figure 1:** Mutual fund fire sales and abnormal stock returns

This figure plots cumulative average abnormal returns (CAARs) in the three quarters before and after mutual fund fire sales (as defined in Section 1.3) for the full sample of firms between 1990 and 2015 and the subsample with credit ratings. Panel A reports CAARs relative to the CRSP equal-weighted index. Panel B reports CAARs relative to characteristic-matched portfolios.

Panel A: Market-adjusted




Panel B: DGTW-adjusted

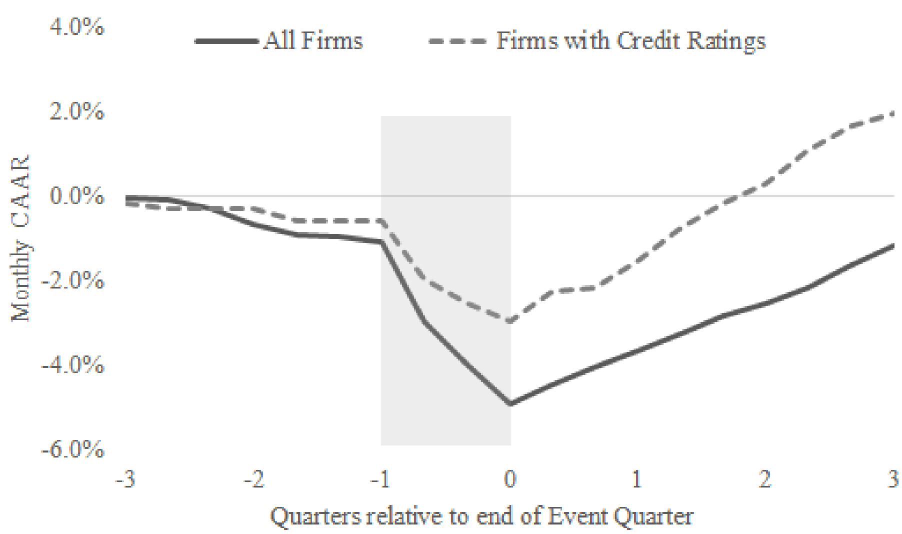
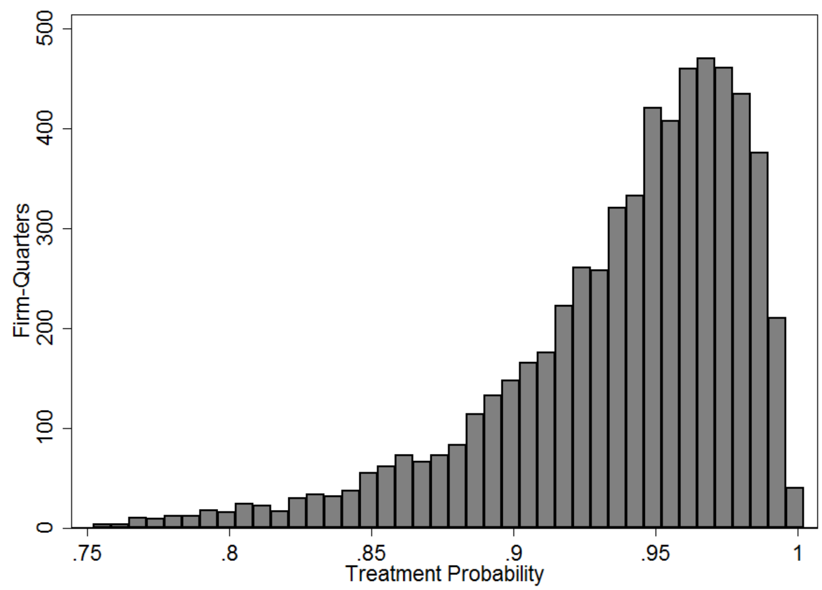


**Figure 2:** Propensity scores

This figure plots the propensity score estimates for being a fire-sale stock (as defined in Section 1.3) from the conditional logit model in Table 2 for the fire-sale firm-quarters ('treated') in Panel A and all others in Panel B. We set year-quarter fixed effects to zero for comparability of scores across time. See Sections 1.3 and 2.2 for details.

Panel A: Treated




Panel B: Not Treated

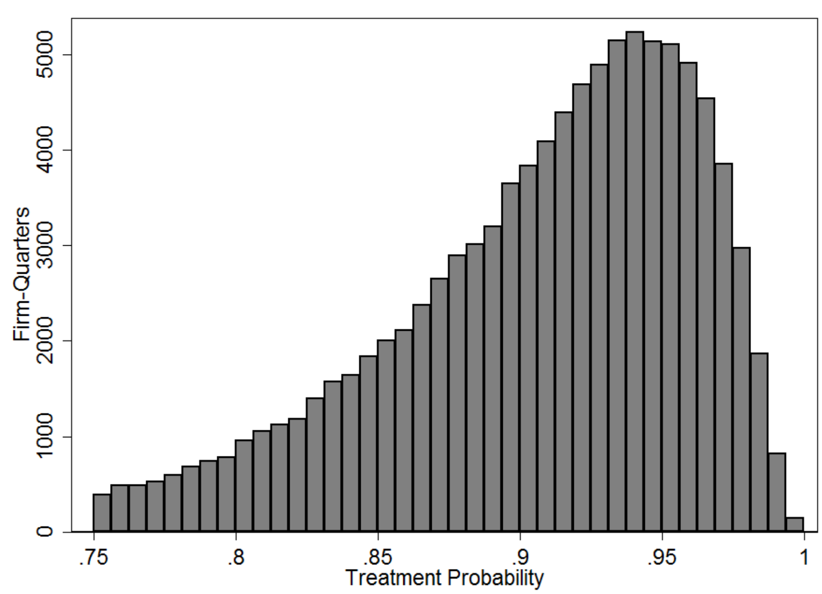
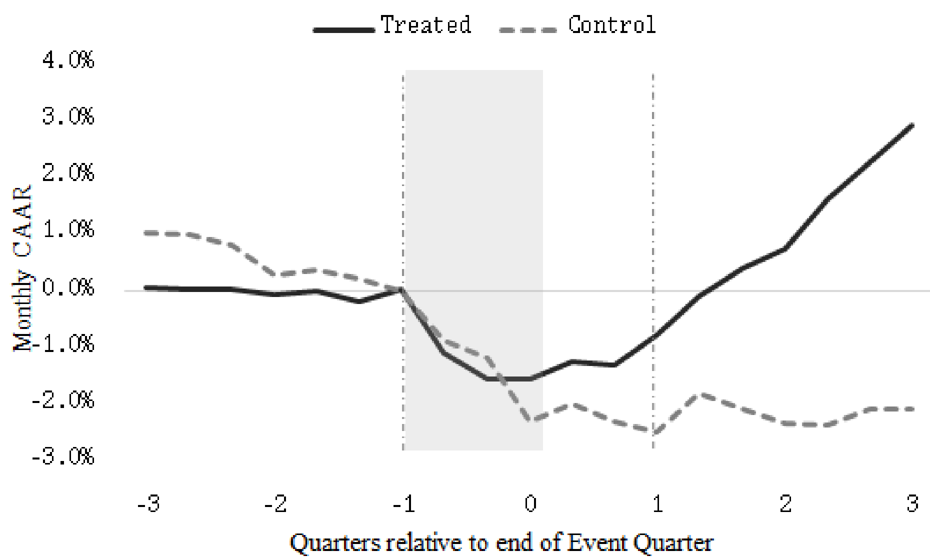


**Figure 3:** The effect of fire sales on stock returns and CDS spreads.

This figure plots cumulative average abnormal returns (CAARs) and CDS spread changes for the fire-sale stocks (as defined in section 1.3) and matched controls during the 2002-2015 period. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of *EQ*), and return in *EQ*. See Section 1.2 for details. Panel A reports CAARs relative to characteristic-matched portfolios. Panel B reports CDS spread changes for the same set of treated and control firms. The shaded area is the Event Quarter (*EQ*), and the area between vertical dashed lines are the 6 months starting at the beginning of EQ.

Panel A: Cumulative DGTW-adjusted stock returns




Panel B: Cumulative changes in CDS spreads

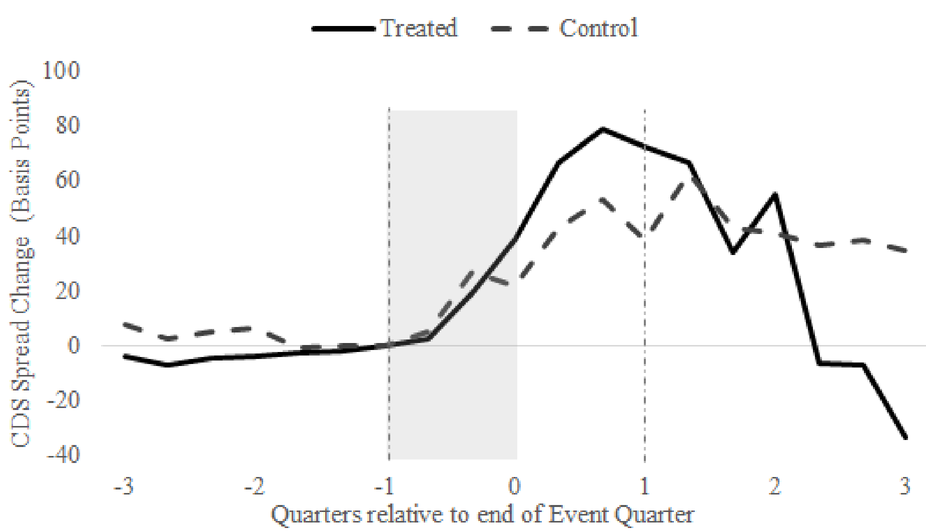
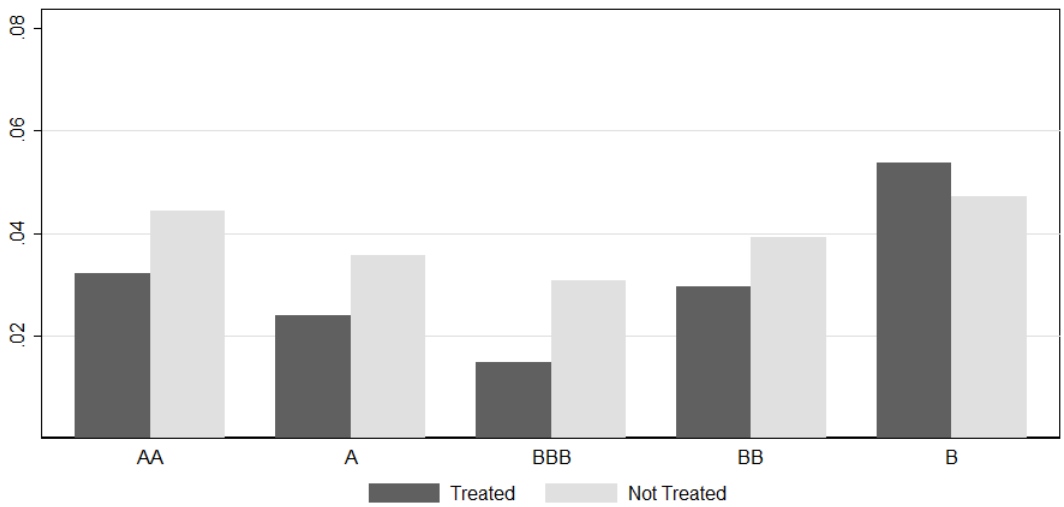


**Figure 4:** Fire-sale effects by rating level

This figure compares credit rating downgrade probability for fire-sale stocks (as defined in Section 1.3) to all other stocks (Panel A), and to matched controls (Panel B) by rating category.Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of $EQ$), and return in $EQ$. See Section 1.2 for details.

Panel A: Full sample




Panel B: Matched sample

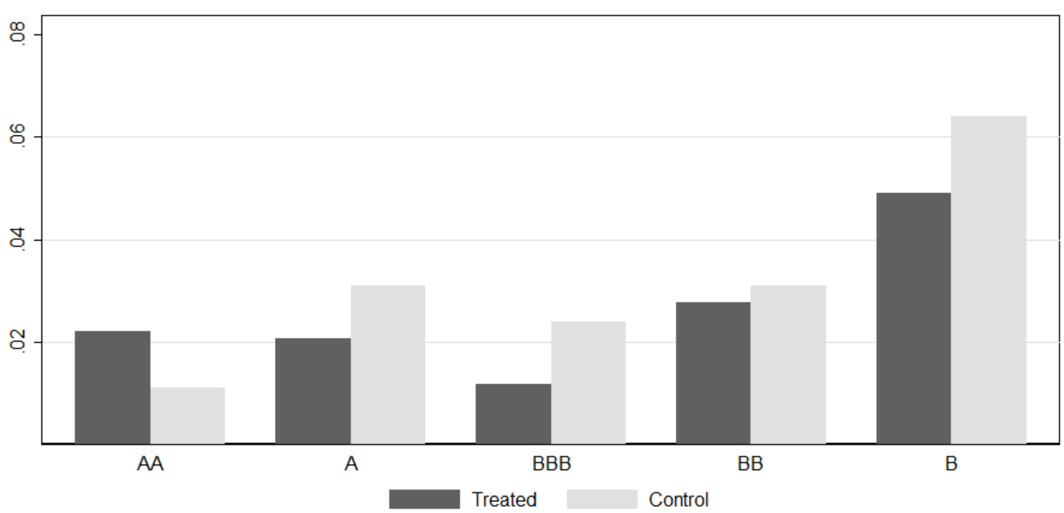
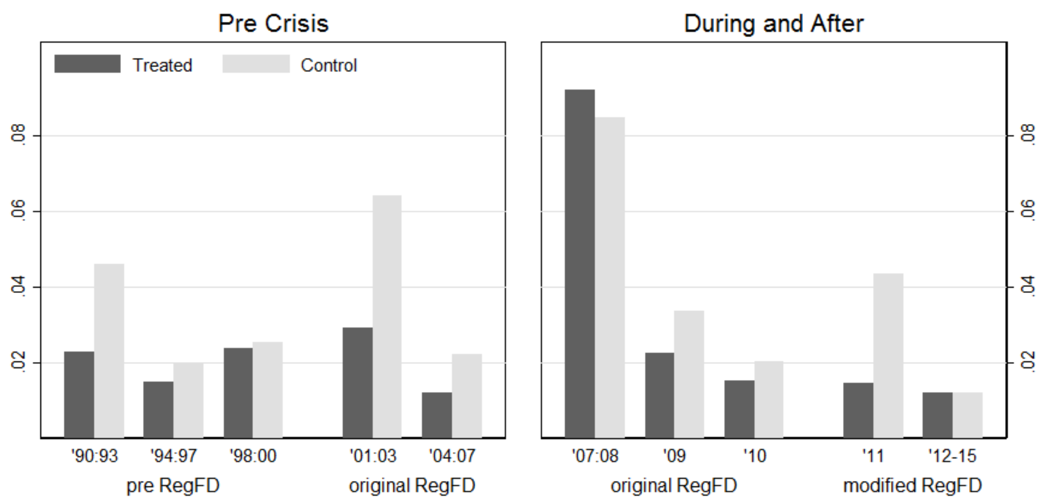


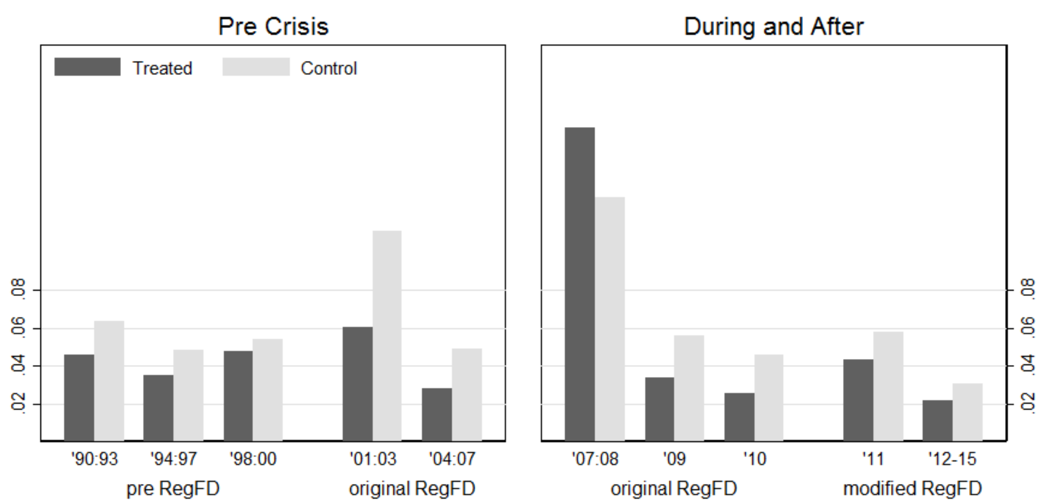
**Figure 5:** Fire-sale effects over time

This figure compares realized rating downgrade probabilities for fire-sale stocks (as defined in section 1.3) to matched controls over different time periods and regulatory regimes. Each treated firm-quarter is matched to a control by credit rating, industry, propensity to experience fire sales (all as of the start of *EQ*), and return in *EQ*. Panel A reports results for the Event Quarter (*EQ*), whereas Panel B reports results for 6 months starting at the beginning of *EQ*.

Panel A: Event Quarter (EQ) ATT




Panel B: EQ to EQ+1 ATT



## Appendix A. Data used to compute fire sales

This section describes the data used to calculate mutual fund fire sales. The CRSP Survivorship Bias Free Mutual Fund database provides data at the mutual fund share class level. We use the MFLINKS file provided by Wharton Research Data Services (WRDS) to aggregate data to the fund level. For any observations not matched to MFLINKS, we use the CRSP portfolio number to aggregate the different share classes. We then merge the CRSP mutual fund database with the Thompson Financial CDA/Spectrum holdings database.

Our mutual fund sample includes only equity mutual funds. Following Coval and Stafford (2007), we exclude funds with the following Investment Objective Codes: international, municipal bonds, bond and preferred, or metals. We also exclude sector funds that specialize in specific industries by removing funds with Lipper classification codes AU, H, FS, NR, RE, TK, UT, CG, CMD, CS, ID, BM, or TL, or Strategic Insight codes GLD, HLT, FIN, NTR, RLE, TEC, UTI, or SEC, or Wiesenberger objective codes GPM, HLT, FIN, ENR, TCH, or UTL.

### *A.1. Additional results*

**Table A1:** Covariate balance for treated firms and controls: Conditional on no recovery subsample

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls. Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of $EQ$), and return in $EQ$. See Section 1.2 for details. We limit the sample to treated firms whose returns did not meaningfully recover during $EQ$. Specifically, we require that cumulative $EQ$ returns are within 1/5 of the minimum return with $EQ$. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest.

**Panel A:** Propensity-score contributors

| | N(Treated)=967, N(Control)=928 | | | | |
|---|---|---|---|---|---|
| | Means | | | St.Deviations | |
| | Treated | Control | P-value | Treated | Control |
| | (1) | (2) | (3) | (4) | (5) |
| MCap(USD bln) | 2.074 | 2.441 | 0.074 | 5.613 | 5.395 |
| Debt-to-EV | 0.400 | 0.386 | 0.391 | 0.209 | 0.223 |
| Mutual Fund Ownership | 0.124 | 0.128 | 0.633 | 0.089 | 0.092 |
| Rating Change past 3 months | -0.029 | -0.005 | 0.270 | 0.454 | 0.465 |
| Rating Change past 12 months | -0.091 | -0.058 | 0.432 | 0.796 | 0.837 |
| Return past 3 months | 0.025 | 0.023 | 0.855 | 0.170 | 0.178 |
| Return past 12 months | 0.130 | 0.140 | 0.589 | 0.355 | 0.383 |
| Realized Volty past 3 month | 0.067 | 0.064 | 0.186 | 0.041 | 0.035 |
| Amihud Ratio | 0.044 | 0.033 | 0.017 | 0.062 | 0.055 |

**Panel B:** Other variables of interest

| | | Means | | | St.Deviations | |
|---|---|---|---|---|---|---|
| | | Treated | Control | P-value | Treated | Control |
| | | (1) | (2) | (3) | (4) | (5) |
| pre EQ | CAPM β | 0.888 | 0.906 | 0.694 | 0.587 | 0.626 |
| | Book-to-Market | 1.104 | 0.907 | 0.007 | 1.671 | 1.302 |
| | Book leverage | 0.447 | 0.451 | 0.791 | 0.287 | 0.279 |
| | CHS Default Prob. | 0.067 | 0.063 | 0.276 | 0.074 | 0.065 |
| EQ | Raw Return | -0.000 | 0.002 | 0.591 | 0.156 | 0.156 |
| | Excess Return (Mkt) | -0.029 | -0.027 | 0.635 | 0.141 | 0.141 |
| | Excess Return (DGTW) | -0.027 | -0.023 | 0.511 | 0.134 | 0.136 |
| | CHS Default Prob. | 0.073 | 0.074 | 0.817 | 0.100 | 0.109 |
| 9-mo after EQ | Cumulative Return | 0.157 | 0.086 | 0.002 | 0.388 | 0.372 |
| | Cumulative Return (vs Mkt) | 0.072 | 0.001 | 0.003 | 0.370 | 0.349 |
| | Cumulative Return (vs DGTW) | 0.037 | -0.023 | 0.001 | 0.330 | 0.319 |
| | CHS Default Prob. | 0.079 | 0.086 | 0.394 | 0.129 | 0.153 |

**Table A2:** Fire sales and credit ratings: Conditional on no recovery subsample

This table examines the effect of mutual fund fire sales on credit ratings. Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of *EQ*), and return in *EQ*. See Section 1.2 for details. We limit the sample to treated firms whose returns did not meaningfully recover during *EQ*. Specifically, we require that cumulative *EQ* returns are within 1/5 of the minimum return with *EQ*.. Column (3) presents 'Average Treatment effect on Treated' (ATT), or the difference in the outcome variable between treated and control firms; a negative number indicates lower mean outcomes for treated firms relative to controls. In Panel A, the outcome variable equals 1 if the credit rating was downgraded during the period and zero otherwise. In Panel B, we take into account the severity of downgrades by reporting the average number of notches downgraded ($E[\#NotchesDown]$). Panel C reports the average number of notches upgraded. The time periods we consider are: *EQ*-2 and *EQ*-1 (the 6 months before the start of *EQ*), *EQ*-1, *EQ*, *EQ*+1, and *EQ* and *EQ*+1 (the 6 months starting at the beginning of *EQ*). Standard errors and t-statistics reported in columns (4) and (5) respectively are robust for heteroskedasticity.

**Panel A:** $Pr\{Downgrade\}$

| | N(Treated) = 4949 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| (EQ-2,EQ-1) | 0.040 | 0.040 | 0.0002 | 0.0037 | 0.06 |
| EQ-1 | 0.021 | 0.022 | -0.0018 | 0.0027 | -0.69 |
| Event Quarter | 0.023 | 0.033 | -0.0103 | 0.0030 | -3.48 |
| EQ+1 | 0.025 | 0.037 | -0.0113 | 0.0031 | -3.61 |
| (EQ,EQ+1) | 0.047 | 0.065 | -0.0184 | 0.0041 | -4.46 |

**Panel B:** $E[\#NotchesDown]$

| | N(Treated) = 1539 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.125 | 0.104 | 0.0208 | 0.0193 | 1.08 |
| EQ-1 | 0.064 | 0.060 | 0.0045 | 0.0132 | 0.35 |
| Event Quarter | 0.072 | 0.112 | -0.0396 | 0.0150 | -2.65 |
| EQ+1 | 0.105 | 0.147 | -0.0422 | 0.0326 | -1.29 |
| EQ and EQ+1 | 0.173 | 0.251 | -0.0786 | 0.0372 | -2.11 |

**Panel C:** $E[\#NotchesUp]$

| | N(Treated) = 1538 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.070 | 0.086 | -0.0156 | 0.0107 | -1.46 |
| EQ-1 | 0.042 | 0.044 | -0.0013 | 0.0079 | -0.17 |
| Event Quarter | 0.034 | 0.029 | 0.0046 | 0.0066 | 0.69 |
| EQ+1 | 0.031 | 0.014 | 0.0169 | 0.0072 | 2.34 |
| EQ and EQ+1 | 0.064 | 0.044 | 0.0208 | 0.0093 | 2.24 |

**Appendix B. Variables Definitions**

**Table A3:** Variable Definitions

| Variable | Description |
|---|---|
| *Rating* | Standard & Poor's long term issuer credit rating or Moody's senior unsecured issuer rating (in Appendix). 21 notches from AAA/Aaa to C, and 1 default category. 'Coarse rating' ignores subcategories (i.e., +/- and 1,2,3), while 'narrow rating' includes subcategories. Changes over the 3- and 6-month horizons are measured relative to the level at the beginning of the period, independently for upgrades (exclude AAA/Aaa) and downgrades (exclude already defaulted). Sources: Compustat, Moody's Corporate Default Risk Service Database. |
| $Pr\{Downgrade\}$ | Realized probability of downgrade computed as a ratio of downgrade events divided by the number of firms in a given period. Multiple downgrades for a firm within the period are counted as one. |
| *E[#NotchesDown]* | The number of notches downgraded divided by number of firms, where notch is a change in a narrow rating category. |
| *E[#NotchesUp]* | The number of notches upgraded divided by number of firms, where notch is a change in a narrow rating category. |
| *Industry* | Fama-French five (Consumer, HighTech, Healthcare, Manufacturing, Other) or twelve (BusEq, Chems, Durbl, Enrgy, Hlth, Manuf, Money, NoDur, Shops, Telcm, Utils, Other) industry classifications based on the company's historical SIC4 code. Sources: Ken French's website, CRSP. |
| *MFFlow* | Mutual fund fire sales defined as the imputed dollar amount sold in a stock by all mutual funds experiencing an outflow $\geq 5\%$ of their assets, normalized by the stock's quarterly trading volume. See Appendix A for details. Sources: CRSP, Thompson Reuters. |
| *Treated* (1/0) | All firm-quarters were *MFFlow* is below the 20th percentile value of the full sample (the global cutoff) and the 10th percentile for that quarter (the local cutoff). |
| *Event Quarter* | The quarter for which the treated firm's *MFFlow* is below the global and local cutoffs. |

**Table A3:** Variable Definitions

| Variable | Description |
|---|---|
| *Control Firm* | Defined for each treated firm. Must have similar characteristics as the treated firm as of the start of the event quarter and the closest return to the treated firm during the event quarter. In particular, (i) the control must be in the same industry as the treated firm, (ii) have a similar propensity to be treated, (iii) the same credit rating at the beginning of the quarter, and (iv) closest stock return during the event quarter. In the main tests, we pick one control within a 2.5% propensity score caliper and also require that the distance in returns is within 2.5%. If a satisfactory match cannot be established within a narrow rating category, we then look for a control candidate within coarse rating category. |
| *Mutual Fund Ownership* | The fraction of a firm's shares outstanding owned by mutual funds. Source: Thomson Reuters. |
| *CHS Default Prob* | Probability of default for month $t$+12 obtained using the model parameter estimates from the 12-month ahead model in Table 4 of Campbell, Hilscher and Szilagyi (2008). |
| *Return (Raw)* | Stock return for the respective period, including dividends. Source: CRSP. |
| *Return (Mkt)* | Stock return, including dividends, minus the total return on CRSP value-weighted index for the same period. Source: CRSP. |
| *Return (DGTW)* | Stock return, including dividends, minus the return on the characteristics-matched portfolio following the methodology of Daniel, Grinblatt, Titman and Wermers (1997). Sources: CRSP. |
| *CAARs* | Cumulative Average Abnormal Return, either relative to CRSP value-weighted index (Mkt) or the characteristics-matched portfolio (DGTW). Cumulative over time, average across firms. Sources: CRSP, Russ Wermers' website. |
| *Realized Variance* | Sum of squared stock returns over the quarter. Source: CRSP. |
| *MCap* | Market value of common equity. End of quarter value. Source: CRSP. |
| *Debt-to-EV* | Book value of long- and short-term debt outstanding divided by the sum thereof and the market value of common equity. End of quarter value. Source: CRSP, Compustat. |
| *Book leverage* | Book value of long- and short-term debt outstanding divided by the sum thereof and book value of common equity. End of quarter value. Source: CRSP, Compustat. |
| *Book-to-Market* | Book value of common equity divided by the market value of common equity. End of quarter value. Source: CRSP, Compustat. |

**Table A3:** Variable Definitions

| Variable | Description |
|---|---|
| *CAPM* $\beta$ | Rolling estimate from monthly stock returns regressed on the value-weighted CRSP returns. At least (most) 12 (60) months required. End of quarter value. Source: CRSP. |
| *Amihud Ratio* | Quarterly average of daily absolute returns to dollar volume traded, winsorized at 0.0001 and 0.3 as in Acharya and Pedersen (2005). Source: CRSP. |
| *CDS spread changes* | The CDS sample is restricted to contracts with 5 years to maturity on names traded in the United States in US Dollars. Monthly CDS spreads are the average of CDS spreads over the last five days of the month. For each firm we choose the contract that is likely to be the most liquid. In particular, we give first preference to contracts whose spreads are based on at least three quotes within the currency group (Composite Fallback level of 'CccyGrp'). If none are available, we prefer contracts with document clause XR or XR14 after November 2010 (the CDS 'Big Bang') and MR before that date. If neither are available, we use contracts with document clause CR or CR14. We compute changes in average monthly spreads within a particular contract type. Quarterly changes are the sum of monthly changes over the quarter. Source: Markit |
| *CDS Implied Downgrades* | Based on ratings implied by five-year CDS contracts on a firm as computed by Markit. |

## Appendix C. Robustness tests using Moody's ratings

### **Table A4:** Covariate balance for treated firms and controls: Moody's sample

This table presents means and standard deviations of selected variables for fire-sale ('treated') stocks and controls .Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of $EQ$), and return in $EQ$. See Section 1.2 for details. The fewer number of control relative to treatment firm-quarters indicates that some controls are matched to multiple treated firms. Panel A reports variables used in the propensity score model while Panel B reports other variables of interest. The sample period is 1990-2008.

**Panel A:** Propensity-score contributors

| | N(Treated)=1203, N(Control)=1153 | | | | |
|---|---|---|---|---|---|
| | Means | | | St.Deviations | |
| | Treated | Control | P-value | Treated | Control |
| | (1) | (2) | (3) | (4) | (5) |
| MCap(USD bln) | 2.041 | 2.088 | 0.881 | 5.331 | 4.592 |
| Debt-to-EV | 0.408 | 0.389 | 0.333 | 0.213 | 0.231 |
| Mutual Fund Ownership | 0.125 | 0.128 | 0.725 | 0.090 | 0.098 |
| Rating Change past 3 months | -0.035 | -0.032 | 0.905 | 0.487 | 0.577 |
| Rating Change past 12 months | -0.115 | -0.077 | 0.324 | 0.850 | 0.895 |
| Return past 3 months | 0.022 | 0.022 | 0.982 | 0.174 | 0.189 |
| Return past 12 months | 0.115 | 0.113 | 0.930 | 0.360 | 0.416 |
| Volatility past 3 month | 0.070 | 0.069 | 0.564 | 0.044 | 0.040 |
| Amihud Ratio | 0.049 | 0.036 | 0.030 | 0.067 | 0.058 |

**Panel B:** Other variables of interest

| | | Means | | | St.Deviations | |
|---|---|---|---|---|---|---|
| | | Treated | Control | P-value | Treated | Control |
| | | (1) | (2) | (3) | (4) | (5) |
| pre EQ | CAPM β | 0.908 | 0.964 | 0.334 | 0.581 | 0.630 |
| | Book-to-Market | 1.115 | 0.921 | 0.036 | 1.680 | 1.388 |
| | Book leverage | 0.461 | 0.462 | 0.913 | 0.290 | 0.293 |
| | CHS Default Prob. | 0.073 | 0.069 | 0.408 | 0.093 | 0.083 |
| During EQ | Raw Return | -0.009 | -0.009 | 0.978 | 0.164 | 0.164 |
| | Excess Return (Mkt) | -0.038 | -0.038 | 0.992 | 0.150 | 0.149 |
| | Excess Return (DGTW) | -0.032 | -0.033 | 0.848 | 0.145 | 0.144 |
| | CHS Default Prob. | 0.084 | 0.083 | 0.940 | 0.126 | 0.130 |
| 9m after EQ | Cumulative Return | 0.151 | 0.099 | 0.018 | 0.396 | 0.373 |
| | Cumulative Return (vs Mkt) | 0.065 | 0.016 | 0.016 | 0.371 | 0.347 |
| | Cumulative Return (vs DGTW) | 0.027 | -0.009 | 0.001 | 0.334 | 0.315 |
| | CHS Default Prob. | 0.082 | 0.089 | 0.264 | 0.130 | 0.145 |

**Table A5:** Fire sales and credit ratings: Moody's sample

This table examines the effect of mutual fund fire sales on credit ratings .Each treated firm-quarter is matched to a control by credit rating, propensity to experience fire sales (all as of the start of *EQ*), and return in *EQ*. See Section 1.2 for details. Column (3) presents 'Average Treatment effect on Treated' (ATT), or the difference in the outcome variable between treated and control firms; a negative number indicates lower mean outcomes for treated firms relative to controls. In Panel A, the outcome variable equals 1 if the credit rating was downgraded during the period and zero otherwise. In Panel B, we take into account the severity of downgrades by reporting the average number of notches downgraded ($E[\#NotchesDown]$). Panel C reports the average number of notches upgraded. The time periods we consider are: *EQ*-2 and *EQ*-1 (the 6 months before the start of *EQ*), *EQ*-1, *EQ*, *EQ*+1, and *EQ* and *EQ*+1 (the 6 months starting at the beginning of *EQ*). Standard errors and t-statistics reported in columns (4) and (5) respectively are robust for heteroskedasticity. The sample period is 1990-2008.

**Panel A:** $Pr\{Downgrade\}$

| | N(Treated) = 1203 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.041 | 0.042 | -0.0017 | 0.0074 | -0.23 |
| EQ-1 | 0.019 | 0.022 | -0.0033 | 0.0054 | -0.61 |
| Event Quarter | 0.027 | 0.037 | -0.0091 | 0.0059 | -1.54 |
| EQ+1 | 0.030 | 0.043 | -0.0133 | 0.0068 | -1.97 |
| EQ and EQ+1 | 0.054 | 0.074 | -0.0200 | 0.0086 | -2.31 |

**Panel B:** $E[\#NotchesDown]$

| | N(Treated) = 1203 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.098 | 0.103 | -0.0050 | 0.0192 | -0.26 |
| EQ-1 | 0.045 | 0.056 | -0.0108 | 0.0135 | -0.80 |
| Event Quarter | 0.067 | 0.094 | -0.0274 | 0.0154 | -1.78 |
| EQ+1 | 0.076 | 0.126 | -0.0499 | 0.0214 | -2.33 |
| EQ and EQ+1 | 0.138 | 0.214 | -0.0765 | 0.0263 | -2.91 |

**Panel C:** $E[\#NotchesUp]$

| | N(Treated) = 1202 | | | | |
|---|---|---|---|---|---|
| | Treated | Control | ATT | SE | t-stat |
| | (1) | (2) | (3) | (4) | (5) |
| EQ-2 and EQ-1 | 0.044 | 0.065 | -0.0208 | 0.0084 | -2.48 |
| EQ-1 | 0.021 | 0.037 | -0.0166 | 0.0056 | -2.96 |
| Event Quarter | 0.029 | 0.022 | 0.0075 | 0.0066 | 1.13 |
| EQ+1 | 0.032 | 0.034 | -0.0017 | 0.0077 | -0.22 |
| EQ and EQ+1 | 0.060 | 0.056 | 0.0042 | 0.0100 | 0.41 |